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THE EFFECTS OF PACING AND DISTRIBUTION ON INTERCORRELATIONS OF MOTOR ABILITIES *

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INTRODUCTION

Research in the field of motor ability has persistently indicated that most intercorrelations between motor skills are low and positive. Motor ability tests ordinarily do not relate closely to one another, nor, unfortunately, to the criteria basic to personnel selection. The fact of low intercorrelation has drawn considerable attention in recent years (**1, 2, 4, 6, 13**). Since an understanding of behavioral laws in any area is simplified by the charting of general factors (in the statistical sense), studies of the interrelationships of motor abilities have shown this same concern. In the opinion of various investigators, certain variables may operate to increase the test intercorrelations and to produce some evidence of general factors: (a) degree of maturity (**5**), if significant for two performances, would tend to increase the *r;* (b) space-perception, and perhaps other intellectual factors, may cause increased intercorrelation (**6, 13**); (c) common patterns of movement or (d) common musculatures, in performance on two tests, may result in some degree of test clustering (**1, 4, 6, 13**).

Although such factors, where operative, have tended to increase test intercorrelations, they are definitely narrow group factors rather than broad or general ones entering into many motor performances. A heterogeneous test battery is, therefore, sure to yield a low average intercorrelation. One of the conclusions reached through a study of the available literature is that the collections of tests employed have been thoroughly heterogeneous; likewise, the conditions under which they have been given are thoroughly variable from one test in a battery to the next. Furthermore, the small number of group factors charted to date, and their narrowness, make it clear that, under the

* This paper is a summary of a Ph.D. dissertation done at the State University of Iowa under the direction of Professor C. E. Buxton.

conditions typically employed, many of the variables influencing test intercorrelations are of unknown significance. It is the purpose of the present study to determine the contribution of some of these hitherto neglected variables to the observed test intercorrelations. In order to do this, it is necessary to extend the list of possible common factors recorded above.

Most motor ability tests involve considerable intra-test learning, and at the same time there are certain possibilities for an influence by fatigue. Exactly how these affect two different tests is evidently, in part, a function of the test administration procedure. If such conditions affect two test performances in a common way, will they affect the test intercorrelations? It will be anticipated that such a question will lead to an experimental attempt to 'manipulate' test intercorrelations on the basis of controlled variation of *procedural* determiners of test score, as contrasted with previous factorial or inspectional analyses of data derived from scores subject to randomly operative procedural variables.

The variables to be investigated.—A reading of earlier studies suggests that two variables, in particular, have contributed to the heterogeneity of administration procedures. They are the degree of distribution and the degree of pacing of practice. Investigators have differed in what they have called distributed practice, but with motor tests some degree of distribution of work leads to equal or better performance than continuous work. This statement is confirmed by studies with the pursuit rotor (**7**, **8**, **9**, **17**), pursuit-meter (**11**), and pursuit oscillator (**16**). Directly applicable here are two studies done for the Army Air Force, with the apparatus employed in the present study. Spence, Buxton, and Melton (**14**) used the S.A.M. (School of Aviation Medicine) rotary pursuit test with four different distributions of work. The most highly distributed practice (10 sec. work, 20 sec. rest) was clearly superior to continuous work. In a second study (**15**), these same investigators used the S.A.M. complex coordination test with four different distributions of practice. No obvious differences appeared before approximately five min. of practice, after which all three distributed groups were slightly superior to the continuous group. Those groups with the shorter work periods were least superior to the continuous; this may have been due to the fact that the work-rest ratio became larger with shorter work periods.

It is probable that the superiority of distributed practice is based upon the magnitude of the work-rest ratio and not upon absolute length of trial or rest taken separately. A smaller work-rest ratio is superior to a larger one within apparently wide limits. However, most of the relevant work with this variable has been done with

pursuit learning. The generalization is not so well substantiated with tests like the complex coordination apparatus.

Little or no work has been done with pacing, and a working definition of the term is needed. Some motor tests operate at a set speed or 'pace' and require *S* to adapt to this speed if he is to score at all. Others have no set pace but may be operated at *S*'s optimum speed, whatever that happens to be. The former would be called paced tests, the latter unpaced or subject-paced tests. Representative of paced tests is the S.A.M. rotary pursuit, of unpaced tests is the S.A.M. complex coordination test.

The problem.—Logically, a continuum could be set up embracing a variety of testing situations. At one end of the continuum would be comparisons between randomly selected tests. The average relationship in such a situation would presumably be quite low, .25 or less. At the other extreme would be two test situations involving almost identical conditions; *r* should then be significantly higher than for 'random' pairing of tests. Of interest in this connection is the work of Campbell (4), who used the same apparatus for different measurements, allowing only the musculature (limb) to vary. Corrected coefficients were in the high .80's and .90's.

The two major variables in the present experiment are pacing and distribution of practice. Despite extensive experimentation on distribution, this variable has not been related to correlations between two or more tests; nor has the variable of pacing. Will these two variables create a common 'factor' influential enough to raise the correlation between two motor tests above what is typically found? The experimental problem is two-fold: (1) If an individual *S* performs on two tests under similar procedural conditions, so far as these two variables are concerned, will there be a closer relationship between his two performances than will be the case when *S* performs on the two tests under dissimilar procedural conditions? (2) How will pacing and distribution affect the learning function for motor tests? That is, does distribution have a different influence for paced work than it has for unpaced work, and is this influence the same for two selected motor tests? It is not likely that a change in the two aspects of test procedure would alter the correlations between the two tests unless it also changed the form of the learning function.

Apparatus and Procedure

The tests.—Two psychomotor tests were employed, the S.A.M. rotary pursuit test (10)[1] and the S.A.M. complex coordination test (10), an adaptation of the Mashburn Serial Reaction Time Apparatus. Dual units of both tests were available.

[1] This reference contains pictures of both the S.A.M. Rotary Pursuit Test and the S.A.M. Complex Coordination Test. It also contains a more complete description of the tests, and wiring diagrams of the control unit for the unpaced rotary pursuit and of the pacing unit for the complex coordination test.

Both tests could be used with continuous practice, but some fairly extreme degree of distribution was desired. The work-rest ratio of 1:2 (10 sec. work, 20 sec. rest) seemed appropriate for the rotary pursuit test (14). In the case of the complex coordination test, it was not feasible to set such an extreme ratio. Due to the nature of the scoring (limited number of matchings), a single work period of less than one min. would not produce a sufficiently reliable score. Also it was not considered desirable to have rest periods of more than one min. since longer pauses might cause *S* to become bored. A work-rest ratio of 1:1 (one min. work, one min. rest) was actually employed.

An attempt was made to secure both paced and unpaced forms of each test. The rotary pursuit test consists of a phonograph-like disc which carries a small metal 'target' and revolves at a constant rate of one r.p.s. It is the task of *S* to maintain contact with the target by means of a flexible metal stylus. A control unit permits the choice of work and rest periods (either continuous or in the 1:2 ratio) and records *S*'s score in units of .001 min. of contact.

For this experiment, it was necessary to construct a new rotary pursuit control unit (10). The two test units themselves were not altered, but were simply plugged into the new control box, and either continuous work or a 1:2 work-rest ratio could be chosen. The crucial way in which the new control unit differs from the old is that it changes the rotary pursuit from a paced to a more nearly unpaced test. That is, it changes it from a test in which the rotor disc runs continuously at a certain rate during a trial, to a test in which the disc does not begin to revolve until *S* actually contacts the target, and coasts to a stop as soon as he loses contact.

The complex coordination test consists of an upright panel on which are mounted three double rows of red and green signal lights. A rudder bar, stick, and seat are installed directly in front of this panel, and it is the task of *S* to bring the green (response) lights into alignment with their respective red (stimulus) lights by use of the stick and rudder. When this matching has been completed, a new red light pattern is automatically presented; *S*'s score is the number of such matchings he can make in a specified interval of time. The presentation of new red light patterns proceeds at whatever rate *S* is able to complete the matchings.

For this experiment, it was necessary to construct a pacing device (10) which would automatically present a new pattern of red lights at equal intervals of time. So far as the test unit itself is concerned, the effect is precisely the same as though *S* were making the matchings. The pacing device consists of a thyratron circuit with a regulated power supply; it is wired into the test circuit and changes the red light pattern every five sec. unless in the meantime *S* has made the matching, in which case a new five-sec. interval begins. Thus, with paced work, *S* is credited with a match only if he can complete it in less time than the pacing unit requires to complete one cycle. There is no apparent upper limit to the number of matchings possible for *S* within a given unit of time; likewise, with the paced test, it would be possible for *S* to perform for the whole practice period without a single matching to his credit.

Preliminary studies.—Two preliminary studies were made. These were carried out to determine whether or not the modifications made in the two psychomotor tests would actually alter the level of performance or the speed of learning. One of these preliminary studies was done with the rotary pursuit test, the other with the complex coordination test. Only a brief summary of these studies appears here.

The preliminary study with the rotary pursuit test employed 80 college women as *S*s. All these women performed on the unpaced test; that is, they performed on the test in which the target moved only when *S* was in contact with it with his stylus. None of these *S*s had ever performed on the pursuit rotor before. It was assumed that the modified (more nearly unpaced) rotary pursuit might give different learning curves, or different mean levels of performance, or both. The results confirmed these expectations. Half of the *S*s had distributed practice (10 sec. work, 20 sec. rest) and half had massed (continuous) practice. A comparison was made between the results obtained here and those obtained by Reyna (12) in a study with the paced rotor using these same work distributions. The data from the Reyna study indicate that paced curves begin relatively low. Initial scores with the unpaced apparatus are much higher than those obtained in the Reyna study. Another difference is that Reyna's distributed group shows steady improvement throughout his work period while his massed group shows only a moderate amount of improvement. In the present study, both curves are relatively flat, and the difference between massed and distributed practice is less than that found in the study by Reyna and in other studies using the paced apparatus. Reyna's data yielded a C.R. of 7.43 for the difference between massed and distributed practice. Although the difference between massed and distributed work was much smaller in the case of the 'unpaced data' obtained in the present study, the scores were less variable, and the difference almost as significant, the C.R. being 5.66.

The preliminary study made with the modified complex coordination test employed 20 college men as *Ss*. The technical changes necessary in order to make the apparatus into a more nearly paced test were difficult, and some little time was spent in perfecting the electronic thyratron circuit which activated the unit at regular intervals. A number of *Ss* were used in attempting to select the most appropriate pacing interval, which turned out to be five sec. Once this interval was selected, the 20 *Ss* referred to above were run. Of these, nine had distributed practice (one min. work, one min. rest) and eleven massed (continuous) practice. It would have been desirable to use more *Ss* than this, but it was more important to conserve the supply of college males. The question here, as with the rotary pursuit test, was whether or not the modifications in the test would have implications for mean scores or slope of the learning curves.

The results obtained indicated that such would be the case. The data from the present study were compared in only a general way with those from another investigation. Using the *unpaced* complex coordination test, Spence, Buxton, and Melton (15) did not obtain clear-cut differences between massed and distributed groups before five min. of practice. This may have been due to two factors, the large work-rest ratios of their distributed groups, or the unpaced character of the test. There was very little difference in 'pressure' between massed and distributed practice, as the test was used by these investigators. The paced form of the test increased this pressure factor, inasmuch as the time a man had spent in attempting to align the lights was lost unless he was able to do it in the specified time.

In contrast to the Spence, Buxton, and Melton (15) study, the present study indicated that distributed practice was significantly superior to massed practice. A final status (eighth min.) difference yielded a t of 2.17. This represents a five percent level of confidence. The results are not completely conclusive, in that the present study employed a smaller work-rest ratio than did the Spence, Buxton, and Melton study, but they do suggest that pacing may be a factor in the relationship between massed and distributed practice.

The preliminary studies indicated, then, that with these two tests, pacing is probably a factor in the superiority of distributed practice, and also in the form which the learning curves take. (The initial scores on the paced complex coordination test are lower than those obtained by Spence, Buxton, and Melton with the unpaced complex coordination test.)

Procedure of the main study.—The preliminary studies indicated the interrelationship of the pacing and distribution variables as far as their influence on test performance was concerned. The major part of the experiment was undertaken to determine whether 'equating' of the two procedural variables would affect the correlations between the tests. In the main experiment, 400 men, mostly college students, were used as *Ss*, and were assigned to experimental conditions in the order of their appearance. Their ages ranged from 16–34 years. None of the men had ever performed on either the rotary pursuit or the complex coordination test before. The *Ss* were divided into eight groups of 50 each to correspond to eight experimental conditions. These experimental conditions are summarized in Table I. As seen in the table, half of the *Ss* took the

TABLE I

EXPERIMENTAL DESIGN FOR THE MAJOR EXPERIMENT

N	Procedure
50	Both tests, Massed-Paced
50	Both tests, Distributed-Paced
50	Both tests, Massed-Unpaced
50	Both tests, Distributed-Unpaced
50	Rotor, Massed-Paced; Comp. Coord., Distr.-Unpaced
50	Rotor, Distr.-Paced; Comp. Coord., Massed-Unpaced
50	Rotor, Massed-Unpaced; Comp. Coord., Distr.-Paced
50	Rotor, Distr.-Unpaced; Comp. Coord., Massed-Paced

two tests under *similar* conditions so far as the two major variables were concerned. The other half took the two tests under *dissimilar* conditions. (Hereafter, these experimental conditions are sometimes abbreviated, e.g., massed-paced is referred to as MP.) *Ss* were used both singly and in pairs. Some attempt was made to equate the eight groups so far as the number of paired

Ss was concerned, but this factor was left partially to chance. The eight experimental conditions were presented in a counter-balanced order. This counter-balancing was also extended to the order of administering the two tests, and, within any one experimental group, approximately half the *Ss* performed on one test first and half on the other.

With the rotary pursuit test, the total amount of practice was always four min., either continuous or in 10 sec. blocks alternated with 20 sec. rest. With the complex coordination test, the total amount of practice was 10 min., continuous or in one min. blocks alternated with one min. of rest. All *Ss* performed on the two tests in immediate succession. During the brief interval between the two performances, no hint was given as to the quality of the first performance.

Results

Levels of performance and learning curves.—The data from the major study indicate that the paced and the unpaced forms of the

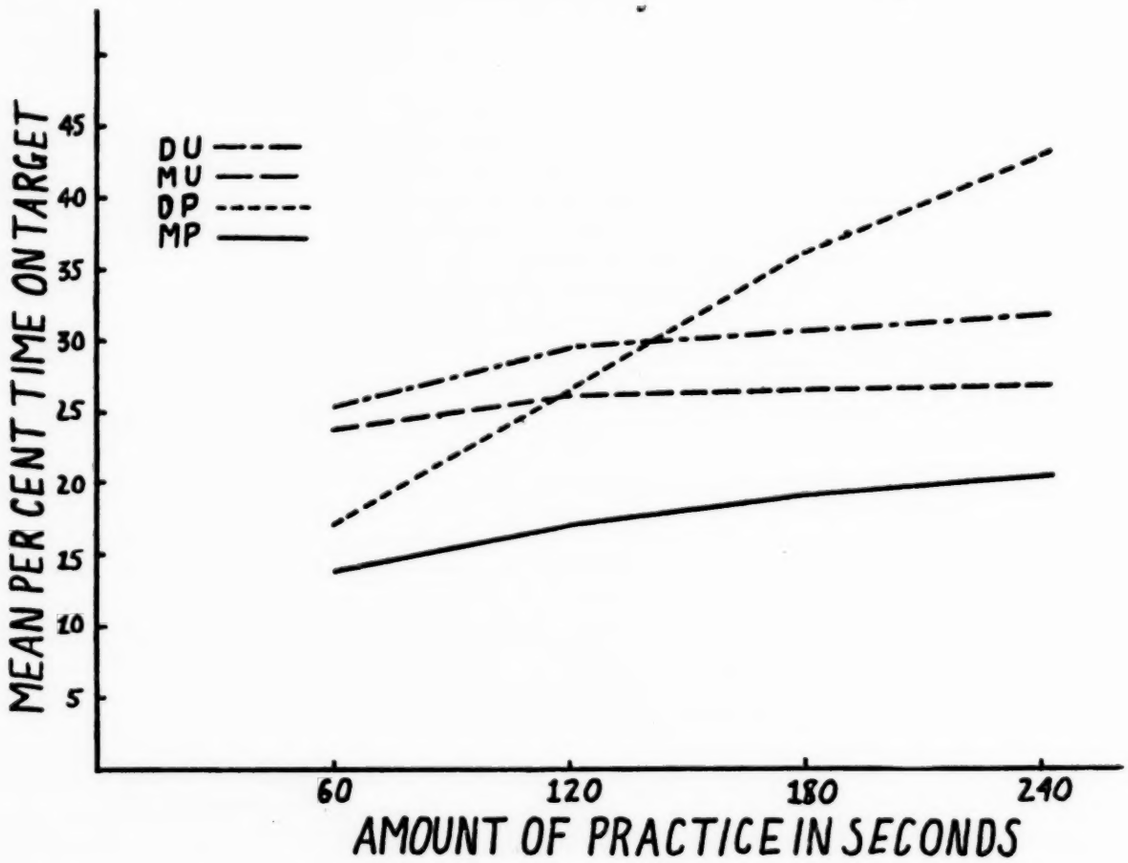

Fig. 1. Learning curves for the unpaced and the paced rotary pursuit tests, major study (N = 100 for each curve)

rotary pursuit are two different tests in the sense that the skill involved shows rather different learning curves. The data for the complex coordination tests show the same trends, but the unpaced and paced forms of this test do not produce learning curves as different from each other as is the case with the rotary pursuit test.

Curves for the rotary pursuit test, representing percent of time on the target, are found in Fig. 1. The preliminary study done with the rotary pursuit test had indicated that scores for unpaced practice

would begin at higher levels than those for paced practice. The major study confirms this. Both unpaced curves (massed and distributed) show, however, only a small amount of gain throughout the total practice period. The massed-paced curve, also, begins to level off rather soon. For both unpaced and paced practice, the superiority of distributed work is evident even during the first min., being much more marked in the case of paced practice.

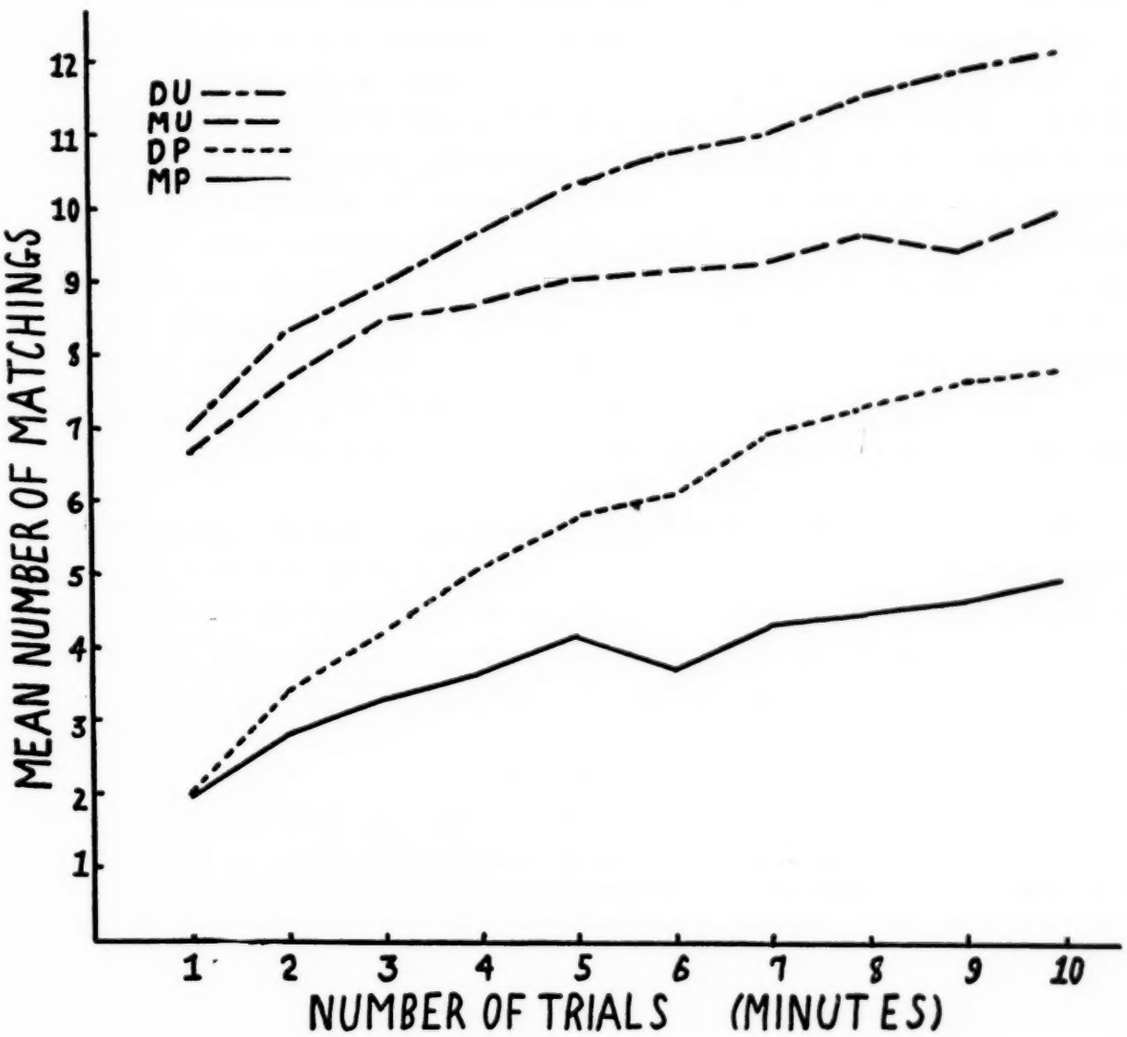

FIG. 2. Learning curves for the unpaced and the paced complex coordination tests, major study (N = 100 for each curve)

The most significant aspect of Fig. 1 is the sharp, steady rise shown by the curve for distributed-paced work. This was anticipated on the basis of data from Reyna's (12) study (reported earlier) and from the study by Spence, Buxton, and Melton (14). Not only does this group surpass the massed-paced group, but, by the end of three min., it has surpassed both unpaced groups. There seems to be no reason to assume, however, that the ultimate ceiling for distributed-paced work would be any different from that for distributed-unpaced

work. The probable reason that the paced curve shows a much steeper rise is that the *Ss* are able to adapt quickly to the rhythm of the revolving disc. With the unpaced rotary pursuit, no such adaptation is possible, and *S* must become quite skilled before anything like a definite rhythm can be established.

The data strongly suggest that paced work on the rotary pursuit test is an initial disadvantage. They indicate, however, that distribution of practice will in time more than offset this initial handicap.

For the data from the complex coordination test see Fig. 2. The scores are in terms of mean number of matchings per min. (trial) of work. These data confirm the preliminary finding that, on this test also, distributed-paced work is significantly superior to massed-paced work. In contrast to the results obtained by previous investigators, the present study also shows distributed-unpaced work to be consistently superior to massed-unpaced work. This superiority increases throughout the practice period. Unpaced work curves on the rotary pursuit tend to be relatively flattened; this is not true with the complex coordination test. As with the rotary pursuit test, pacing on the complex coordination test is an initial disadvantage, the unpaced groups making higher initial scores.

With the complex coordination test, the distributed-paced curve has a steeper slope than any of the others, but by the end of 10 min., it has not reached the level of either of the unpaced curves. However, pacing does increase the superiority of distributed over massed practice. This may be clarified by reference to Table II.

TABLE II

RATIO OF FINAL TRIAL TO INITIAL TRIAL FOR THE FOUR CONDITIONS OF PRACTICE, COMPLEX COORDINATION TEST

Practice Group	Ratio
Distributed-Paced	3.95/1
Massed-Paced	2.52/1
Distributed-Unpaced	1.74/1
Massed-Unpaced	1.51/1

This table does not make a direct, comprehensive comparison among the four types of practice; it simply suggests one of the differences between paced and unpaced work. Obviously, there is a greater superiority shown by distributed practice when the work is paced then when it is unpaced. One aspect of Fig. 2 tends to obscure this. The distributed-unpaced group, by chance, is slightly superior to the massed-unpaced group on trial one. The final superiority of the distributed group in this case is partially an artifact.

It would seem that with a favorable work-rest ratio some superiority for distributed practice will show up even for the unpaced complex coordination test. That it shows up earlier in the present study than in previous investigations may be due to the fact that relatively short work periods were used.

One portion of the original hypothesis is demonstrated. For both the rotary pursuit and the complex coordination test, distribution and pacing are variables which have a definite influence upon the slopes of the learning curves, and the effect of either variable is dependent upon the influence of the other.

Critical ratios.—The comparisons made between the various kinds of practice by a superficial examination of the learning curves are incomplete, and, to supplement them, CR's were computed for both tests using initial status, final status, and total scores. These are presented in Table III. (In order to simplify this table, the means

TABLE III

CRITICAL RATIOS COMPUTED FOR THE DIFFERENCES BETWEEN THE VARIOUS KINDS OF PRACTICE, FOR INITIAL STATUS,* FINAL STATUS,† AND TOTAL SCORE ‡ (N = 100 FOR EACH)

	CR's§		
	Initial	Final	Total
Rotor MP vs. Rotor DP	2.42	*10.21*	7.15
Rotor MU vs. Rotor DU	3.09	*8.64*	6.98
Rotor MP vs. Rotor MU	10.86	4.20	6.98
Rotor MP vs. Rotor DU	12.32	7.76	9.88
Rotor DP vs. Rotor MU	5.02	9.37	3.28
Rotor DP vs. Rotor DU	6.12	6.25	.91
Comp. Coord. MP vs. Comp. Coord. DP	1.91	*6.71*	5.42
Comp. Coord. MU vs. Comp. Coord. DU	2.24	*7.33*	5.40
Comp. Coord. MP vs. Comp. Coord. MU	19.80	15.79	18.59
Comp. Coord. MP vs. Comp. Coord. DU	22.65	21.03	22.91
Comp. Coord. DP vs. Comp. Coord. MU	16.22	5.12	9.86
Comp. Coord. DP vs. Comp. Coord. DU	18.73	9.96	13.85

* Initial status represents the first min. of work on the rotary pursuit and the first three min. of work on the complex coordination test.

† Final status represents the last min. of work on the rotary pursuit and the last three min. of work on the complex coordination test.

‡ The total work period was four min. for the rotary pursuit and 10 min. for the complex coordination test.

§ Five percent and one percent level of confidence are 1.96 and 2.58 respectively.

upon which these CR's are based and the SD's have been omitted.) For the rotary pursuit test, initial and final status scores represent the first and last min. of work respectively. For the complex coordination test, initial and final status scores represent the first and last three min. of work respectively. Attention is called to the ratios in the table which are italicized. They are based upon final status

differences between massed and distributed practice for paced and for unpaced work. These ratios give a somewhat different impression from that obtained from an examination of the learning curves. Figs. 1 and 2 showed the differences between massing and distribution to be smaller for unpaced work, particularly in the case of the rotary pursuit. Due, however, to the smaller variability in unpaced scores, these differences become approximately as significant as those for the paced groups. The reason for the more restricted range of scores in the unpaced groups was not definitely determined.

For both tests, paced and unpaced, the differences between massed and distributed work become more significant with added practice.

Test intercorrelations.—The original hypothesis contained two assumptions, one of them being that pacing and distribution would have some influence upon the slopes of the learning curves. Even if these variables should alter the learning function, however, it would not *necessarily* follow that test intercorrelations would be raised by equating the two variables. It was hypothesized, however, that this *would be the case.* (If a change in the slope of the learning function involved simply a change in the average performance level on both tests, the relative rank of all Ss might remain the same even when the pacing and distribution factors were varied. In other words, pacing and distribution might have implications for group averages but not for individual differences. Even if the effects on the learning functions were precisely the same for both tests, intercorrelations would be no higher for similar conditions than for dissimilar if the pacing and distribution variables had no implications for individual differences.)

The data do not support the hypothesis of increased intercorrelations. Correlations for the similar conditions are lower on the average than the correlations for the dissimilar conditions (see Table IV). This is true with *r*'s obtained for initial status, final status, and total,

TABLE IV

CORRELATIONS BETWEEN ROTARY PURSUIT AND COMPLEX COORDINATION TESTS (N = 50, EACH GROUP)

Experimental Group	Initial	Final	Total
Both tests MP	.06*	.31	.13
Both tests DP	.43	.39	.53
Both tests MU	.27	.43	.45
Both tests DU	.18	.38	.35
Rotor MP—Comp. Coord. DU	.22	.40	.40
Rotor DP—Comp. Coord. MU	.47	.53	.54
Rotor MU—Comp. Coord. DP	.41	.35	.42
Rotor DU—Comp. Coord. MP	.36	.47	.56

* Standard errors for the correlations range from .10 to .14.

these scores having been computed in the same way as those used in the calculation of CR's. The difference in the two sets of correlations is not due to any difference in the variabilities of the scores. Table IV indicates that, in the main, intercorrelations increase with practice. Inasmuch as the SD's are systematically larger for final status scores than for initial scores, the increase in the *r*'s may be due to the increase in the 'range of talent' with added amounts of practice. It is possible that this same factor accounts for the fact that five out of eight coefficients are larger for total scores than for either final status or initial status.

The intercorrelations obtained in this study are somewhat larger than those obtained in previous studies with these two tests. Presumably, this is not due to the effect produced by the major variables and possibly may be attributed to the heterogeneity of the *S*s used here. Buxton and Spence (3) used military personnel and obtained an *r* of approximately .30. The *S*s in the present study were in some cases military personnel, but in most cases civilians, and the scores obtained here perhaps show relatively large variabilities.

A further analysis can be made from Table IV which suggests that *r* may be higher with the 'pressure' off (unpaced tests) and with distributed practice, distribution being a more significant factor in this regard than pacing. Considering only the *r*'s for similar conditions of procedure, and comparing those obtained under pacing with those obtained under unpaced practice, we discover that somewhat higher coefficients are obtained for the unpaced groups. A similar comparison may be made between massed and distributed practice (both tests massed vs. both tests distributed). This comparison reveals higher *r*'s when both tests are presented with distributed practice than when both are used with massed practice. Neither of these differences is statistically significant.

Within the present experimental conditions, pacing and distribution do not, when 'equated,' serve as a general motor 'factor.' Similarities in procedure are seemingly more than offset by such differences as complexity of the task, extent of the musculature involved, etc. It is obvious that pacing has not been made strictly comparable for the two tests. The complex coordination test, when paced, still permits variable response on the part of an individual *S*. That is, he can still do the matchings in one, two, three, four, or more sec. according to his proficiency. The paced rotary pursuit test does not allow for such variable response. More specifically, if *S* can match the lights within the five sec. pacing interval, he perhaps feels very little pressure at all. Distribution, also, is not precisely equated, and it is possible that this variable is not as effective with the complex coordination test as with the rotary pursuit.

If two tests were selected which were qualitatively more equivalent, the pacing and distribution variables could be more nearly equated, and the effects of similarity in pacing and distribution could be more definitely determined. This might involve selecting tests which, from an *a priori* analysis, seemed to be more similar to each other from the standpoint of the particular abilities required. On the assumption that the present findings are valid, the problem could turn out to be rather difficult; such tests might correlate highly without having the pacing and distribution variables equated. If this were true, the net effect of equating the two procedural variables might be slight so far as the intercorrelations were concerned.

Reliabilities of the tests.—Odd-even reliability coefficients, each based on an N of 100, were computed for the four varieties of practice used. For example, a reliability coefficient was computed for rotary pursuit, massed-paced. For this purpose, two groups of 50 records were combined. This included *S*s who had performed on both tests under the massed-paced condition and *S*s who had had massed-paced work only with the rotary pursuit. The same procedure was used to compute the other reliability coefficients. As in previous experiments with these tests, reliabilities are, in the main, very high (see Table V).

TABLE V

RELIABILITY COEFFICIENTS FOR ROTARY PURSUIT AND COMPLEX COORDINATION TESTS (N = 100 FOR EACH COEFFICIENT)

	Paced		Unpaced	
	Rotor	Comp. Coord.	Rotor	Comp. Coord.
Massed	.97	.87	.84	.93
Distrib.	.98	.93	.93	.94

For both tests, they are slightly lower with the modified apparatus. The reliabilities are slightly higher for paced work than for unpaced and are higher for distributed practice than for massed practice.

SUMMARY

1. Technical changes were made in the S.A.M. rotary pursuit test so that the bakelite disc revolved only when *S* was in contact with the target with his stylus. The method of scoring remained the same: percent of time on the target out of the total practice period. Using this unpaced rotor, records were obtained from 80 college women, half of whom practiced continuously for four min., and the other half for four min., with 20 sec. rest periods between each 10 sec. work period. When a comparison was made with previous studies

using the paced rotor with the same work-rest ratios, the following conclusions were reached: the unpaced rotary pursuit yielded much flatter learning curves, less variability in scores, and less difference between massed and distributed practice. Because of the smaller variabilities, however, this difference was almost as significant as the one for paced practice in the Reyna (**12**) study.

2. In addition, the S.A.M. complex coordination test was altered by use of a thyratron circuit which acted as a pacing device. This thyratron circuit caused the pattern of red (stimulus) lights to be automatically changed at regular intervals unless *S* could make the correct matching first. Twenty male *S*s were used in the preliminary study with this apparatus. A significant difference was obtained between massed and distributed practice, the latter involving one min. work, one min. rest. This was a 'final status' difference. No direct comparison was made with previous studies done with the unpaced complex coordination test. However, since previous studies with this test had not revealed significant differences for massed and distributed practice, it was assumed that pacing had produced a real difference in the efficacy of distributed practice.

3. In the main experiment, both the rotary pursuit test and the complex coordination test were employed with 400 male *S*s, mostly university students. In addition to the modified apparatus described above, the two tests were used in their conventional forms. The work-test ratios were the same as those employed in the two preliminary studies. The major experiment tends to bear out the results of the preliminary studies, so far as the effects of pacing and distribution on the learning functions are concerned.

4. Pacing the rotary pursuit test produces lower initial scores. The unpaced groups do not show much improvement, however, nor does the massed-paced group. The distributed-paced group shows rapid improvement which has not leveled off at the end of four min. With the complex coordination test, pacing also produces lower initial scores. The difference is that, on this test, neither paced group surpasses either of the unpaced groups, although the distributed-paced group shows more tendency to do so. With this test, also, although it is not as obvious as with the rotary pursuit, distribution shows more superiority when the work is paced than when it is unpaced.

5. The original hypothesis concerning an increase in the correlations between the two tests is not supported. The correlations based upon opposite conditions of procedure are slightly higher than those based upon similar conditions. This is true for *r*'s based upon initial status, final status, and total scores. Intercorrelations range from .06 to .56 and average approximately .40. They are somewhat

higher than previous investigators have obtained with these two tests. It is concluded that equating the pacing and distribution variables for two motor tests carries, under the present experimental conditions, no implications for a general motor ability.

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EXPERIMENTAL EXTINCTION AS A FUNCTION OF THE DISTRIBUTION OF EXTINCTION TRIALS AND RESPONSE STRENGTH

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I. Introduction

The phenomenon of extinction, the decrement and final disappearance of a conditioned response resulting from repeated non-reinforced presentations of the conditioned without the unconditioned stimulus, has led to some of the most vigorous and interesting theoretical disagreements in the field of learning. Two theoretical positions with respect to the phenomenon of experimental extinction have dominated the thinking in this field. One is the theory proposed by Pavlov, which involves the use of the concept of internal inhibition; the other position may be termed the interference theory and features the concept of competition between responses.

According to the Pavlovian theory, during the process of experimental extinction the observed conditioned response becomes weakened because of the development of internal inhibition. Internal inhibition is regarded by Pavlov (**16**) as a labile, short-lived hypothetical process or state, which develops in the cortex with failure of reinforcement and acts to offset or counteract the positive excitatory state which the conditioned stimulus possesses as the result of prior conditioning. With the passage of time, the inhibitory state is assumed to dissipate itself with the result that the effective excitatory strength of the conditioned stimulus again becomes suprathreshold and the response reappears. The latter phenomenon is known as spontaneous recovery, and several experimental studies have provided us with a picture of the course of the recovery (**9, 10, 11, 19**).

The second type of theoretical interpretation of the phenomenon of experimental extinction, the interference theory, states that the decrement in the conditioned response results from interference by some other competing response. Several variations of the interference interpretation of extinction phenomena have been offered.

[1] The present paper is a portion of a dissertation presented to the Faculty of the Graduate College of the State University of Iowa in partial fulfillment of the requirements for the degree of Doctor of Philosophy. The writer is deeply indebted to Professor Kenneth W. Spence, under whose direction the research was conducted, for many suggestions and criticisms throughout the course of the investigation.

Culler (**2**) states that "extinction occurs when the pattern, though still intact, is not being adequately activated. The previous pattern is no longer being activated because another pattern has arisen during extinction." According to this formulation the competing response pattern of 'not responding' arises during extinction, the motivation which gives rise to the response being a 'labor-saving propensity.' During the course of extinction, the excitatory strength of the 'no-response' pattern becomes dominant over the excitatory strength of the original conditioned response, thereby producing the phenomenon of experimental extinction.

In another form of this interpretation, Wendt (**27**) holds that the phenomenon of experimental extinction is an instance of the competition between response systems. He assumes that all behavior is organized into response systems; a response occurs when it is dominant over the response systems with which it is in competition. According to this formulation the organism ceases to make the conditioned response *when* some other reaction takes its place, whereas according to the Pavlovian formulation the organism ceases to make the conditioned response *and* another response takes its place.

According to Guthrie's (**5, 6**) version of the interference theory, experimental extinction involves learning to make a new response to the stimulus, which is incompatible with and hence displaces or inhibits the old response. The essential condition for such inhibitory learning is for the stimulus to occur and the response to fail to take place. Guthrie (**6**) lists four specific conditions which will result in response decrement. These involve presentation of the stimulus either: (1) in a subthreshold form, (2) when the *S* is too fatigued or exhausted to respond, (3) during the refractory phase of the response, or (4) when other stimuli which evoke incompatible responses are present. Inasmuch as Guthrie assumes that conditioning is always complete after one trial, in each of these instances the component stimulus cues present become associated to their full extent with whatever response does take place, and hence are alienated completely from the original response. In order to account for the spontaneous recovery of the response following experimental extinction, Guthrie adopted the theory of Winsor (**28**).

An experimentally testable deduction which can be drawn from these two opposing theoretical interpretations has to do with the effect of varying the temporal interval between non-reinforced trials upon the rate of development of extinction. According to the Pavlovian theory the decrease of time between trials should facilitate the extinction process. This follows from the assumption of the dissipation of inhibition with time; the shorter the inter-trial period the less time available for the dissipation of the inhibitory state and hence

the more rapidly will this inhibition develop and thus produce extinction. The interference theories on the other hand would predict that the decrease of the inter-trial period would cause a slowing up of the process of extinction, for such extinction (according to these theories) is dependent upon the learning of a new, competing response and it has been amply demonstrated that distribution of trials facilitates the acquisition of a response (**8, 10, 12, 24**).

II. Empirical Studies Pertaining to the Problem

The experimental evidence available concerning the efficacy of 'massing' and 'distributing' the extinction trials is meager and contradictory. In the field of classical conditioning Pavlov (**16**, p. 52) cites data from an experiment performed in his laboratory which is offered as evidence that the rate of extinction is directly related to the temporal rate of elicitation of the non-reinforced trials; i.e., the faster the rate of elicitation the faster the occurrence of extinction. Razran (**22, 23**) cites other experiments performed in Russian laboratories which support Pavlov's generalization of faster elicitation, faster extinction. Hilgard and Marquis (**9**) report data based on the conditioned lid response which also support the Pavlovian generalization that the shorter the temporal period, the faster the speed of extinction. Guthrie (**5**) rearranged the data cited by Pavlov (**16**) so that reduction of response was expressed in terms of the number of non-reinforced trials and thereby demonstrated that in these particular data at least, the temporal length of the inter-trial period had no demonstrable effect on the rate of extinction.

Porter (**20**) in a study of the conditioned eyelid response of human subjects presents data which he interprets as failing to support the Pavlovian conclusion that the faster the rate of elicitation of the non-reinforced response, the fewer the number of trials required to extinguish the response.

In a second study Porter (**21**) compared the speed of extinction of a simple locomotive habit in rats under three different inter-trial time intervals and found no differences. On the basis of these results Porter concluded that the temporal massing of trials does not facilitate the process of extinction.

Gagne (**3**) has reported an experiment of the effect of spacing trials on the acquisition and extinction of a simple conditioned locomotive response in the white rat. Fifteen acquisition trials were given the animals in which the magnitude of the reinforcing object was kept constant. The temporal intervals between trials for the six groups of *Ss* were varied from one-half to five min., and were the same for both the acquisition and extinction trials. While the mean log latencies for the first five extinction trials of each group approached essentially the same value, as Gagne points out, the acquisition curves indicate that each group started the non-reinforced series at entirely different levels of response latency. These differences in the initial strength of the habit undergoing extinction thus obscure the relationship between trial intervals and rate of extinction.

In a second experiment (**3**), Gagne simultaneously varied two experimental variables, magnitude of the reinforcing object and inter-trial interval.

The inter-trial intervals in this experiment were the same as those used in the first experiment. The extinction curves during the first five trials exhibit a well-defined trend towards faster extinction with a decreased inter-trial period. The presence of the added variable of food magnitude in this experiment makes difficult, however, any interpretation of the results in terms of the temporal period between non-reinforced trials.

III. The Problem

The primary objective of the present experiment was to study the experimental extinction of an instrumental conditioned response as a function of the temporal spacing of the successive non-reinforced trials. The progress of extinction was compared under two inter-trial temporal intervals, 10 sec. and 90 sec. The influence of this temporal variable on rate of extinction was studied under different conditions of response strength as determined by the manipulation of two further experimental variables, (1) the number of reinforced trials during the acquisition period and (2) the number of hours of food deprivation at the time of extinction. Four different combinations of these two variables, 10 reinforcements and six hours deprivation, 10 reinforecments and 30 hours deprivation, 60 reinforcements and six hours deprivation, and 60 reinforcements and 30 hours deprivation, provided the different conditions of response strength at which the effect of massing was investigated.

Secondly, the experimental situation was designed to permit the recording of struggling, withdrawing, or agitated behavior on the part of the animal during conditioning and extinction and thus to provide an indication of what role, if any, such activity plays in these processes.

Finally, it was desired to investigate the technique of conditioning employed, particularly since it made possible discrete trials with control of inter-trial intervals and measurement of the development of conditioning in terms of latency. In both of these respects it provides a means of studying instrumental conditioning which is more similar to the methods employed in classical conditioning than the standard Skinner box technique.[2]

IV. Method

A. *Subject.*—The *S*s used in the experiment consisted of 185 albino rats from the colony maintained by the Psychology Department of the State University of Iowa. There were 119 males and 66 females. Of this group 34 males and 18 females were eliminated during the course of the pre-conditioning training. The final number of animals upon which the data are based, therefore, consisted of 48 females and 84 females, a total of 132 animals. The age of the animals when they were started in the experiment ranged from 78 to 115 days with a mean age of 92 days.

B. *Apparatus.*—The conditioning apparatus used in the experiment has previously been described by Bijou (1). Fig. 1 shows its essential features.

The animal's head was placed in a stock (S) which was locked by means of a thumb screw. A closely fitting wire cage (C) was fitted over the body of the animal so as to permit it to remain in a natural sitting position. The animal's head was directly over the food cup shown in front of the stock, and approximately ¾ in. below the response bar (B). The slide (HS) upon which the food cup, stock and body cage were located is shown in the position used during the trials. By means of the control cord (CC) the slide could be moved back two in. into the between-trial position. As the slide moved forward at the initiation of a trial it first closed a switch turning on a light that constituted a part of the conditioned stimulus pattern and then closed a second circuit that activated an electromagnetic marker on a polygraph. The latter was coordinated with the arrival of the animal at a position in which it could touch the response bar with its head. It thus recorded the time from the initial availability of the bar to the time when the slide was withdrawn. Upon being struck from below, the response bar flipped upward and closed a mercury switch. The latter actuated the food dispenser which released a pellet of food into the cup and at the same time closed marker one. Thus, the latency of the response, or the time between the availability of the bar and the striking of the bar by the animal could be accurately recorded.

[2] At the time this experiment was carried out (1941–42) no studies in which this discrete trial method of using the Skinner box technique had been reported. Since then Perin (18) has reported a study in which a discrete trial method was used.

The stock was mounted in pin pivots three in. from the front of the platform. Screw blocks were so adjusted as to permit the stock to move backward ¼ in. from a vertical position.[3] This amount of movement closed the switch (WS) which activated a third marker on the polygraph. Small coil springs (ST) were mounted on either side of the stock to maintain the stock in a vertical position. A minimum pull of eight gm. was required to close the switch (WS). This arrangement permitted the recording of any 'withdrawal' responses made by the animal while it was in the experimental apparatus.

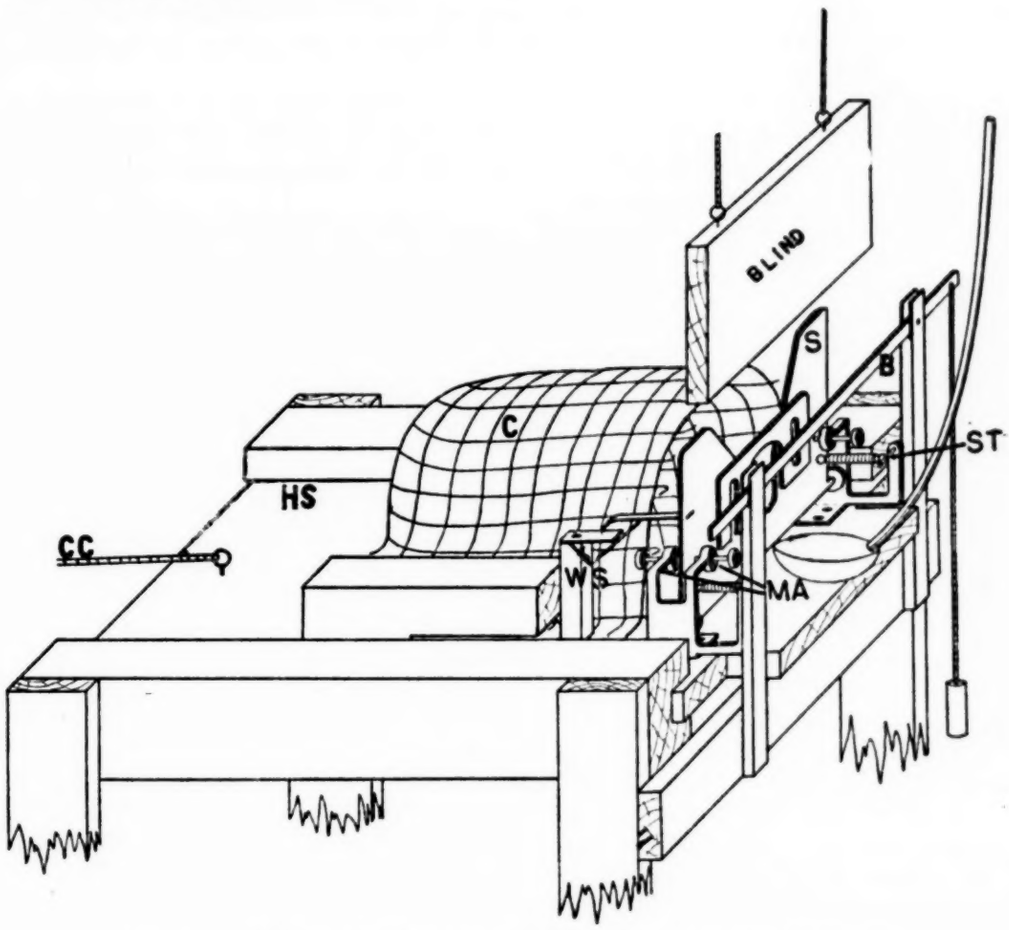

FIG. 1. Diagram showing the essential features of the apparatus

The apparatus was located in a small darkened room which minimized ordinary extraneous visual and auditory stimuli coming from other parts of the building. During the experimental sessions, *E* was located in the adjoining room from where the platform of the apparatus was manipulated. The brightness values of the opal glass located at the front of the light tunnel remained constant at 10.42 apparent foot candles as measured by a Leeds and Northrup Illuminometer.

The pellets of food released by activation of the food dispenser were made from finely ground dog chow, flour and water. The mixture was moulded into a cylindrical shape, the diameter of which was ⅛ in. After drying, the long strings of food were cut into pellets seven mm. long. The resulting pellet weighed about 0.053 gm. The pellets used in the pre-conditioning training were identical to the pellets used in the conditioning series with the one exception that they were not cut as accurately as the experimental pellets.

Special feeders were used for pre-training the animals prior to the conditioning procedures and for feeding the animals at the end of each experimental session. These feeders, located in a room adjoining the experimental room, were constructed on an independent base upon which was mounted a food cup, a plyboard stock, and a body cage.

[3] The distance of the movements of the stock was based upon measurements of the arc performed at the extreme free end of the fulcrum.

C. *Procedure.*

1. *Preliminary training.*—The routine of the pre-conditioning training may be conveniently broken up into four phases. The first phase, concerned with the preliminary adjustment of the animal to the feeder, occupied three days. All of the animals had been deprived of food for 24 hours when this training was begun, and during the course of it were fed only in the feeders or in the experimental apparatus. Each animal was placed in a feeder with the neck piece of the stock securely fixed. A wet mash made of dog chow meal and water was placed in the food dish. The animal was left in the feeder for one hour and then returned to its home cage. At the end of the three-day period all of the animals (i.e., 31) that had failed to start eating the mash were eliminated from the experiment.

The second phase of the pre-conditioning training lasted from the fourth to the 12th day. During it, pellets were substituted for the wet mash and the animals were allowed to eat until they were satiated. Satiation was defined as that point at which the animal stopped eating and failed to start again in a five-min. interval.

The third phase of pre-training consisted in adjusting the animals to the experimental apparatus. This period lasted from the 12th to the 16th day. During this phase the animal, when first brought from the living quarters, was placed in the apparatus and left there until it had eaten all of the food in the cup (approximately three gm), after which it was removed from the apparatus, placed in the feeder and allowed to eat to the usual criterion. At the end of the 15th day all of the animals which exhibited 'poor' adjustment as judged from the criterion of squealing, urination, defecation and spasmodic eating were eliminated from the experiment. A total of 19 more animals were eliminated at this point of the training.

The fourth phase of the pre-conditioning training consisted in adjusting the *S*s to the experimental procedures. On the 16th, 17th, and 18th days the animals were placed in the apparatus with food in the food cup. On the first of these days as soon as the animals started to eat, the platform was moved from the 'between trials' position to the 'in trials' position, and then slowly returned to the 'between trials' position. This procedure was repeated four times. On the 17th day the movement was continued until it no longer served as an 'external inhibitor' or distractor of the eating behavior. On the 18th day 10 further such movement sequences were given. After the *S* had finished the three gm. of food in the apparatus, it was again placed in the feeder. On the 19th day the animals were placed in the apparatus with the food cup empty. The platform was moved to the 'in trials' position and a pellet of food was manually released from the mechanism. The platform was then moved back to the 'between trials' position. As soon as the animal had eaten the pellet of food, the process was repeated with a second pellet being delivered to the food cup. In this manner, a total of 15 pellets was given each *S*, after which it was placed in the feeder and allowed to eat to the satiation criterion. At the conclusion of the 19th day the pre-conditioning training was assumed to be complete. The animals were then assigned to a particular experimental group through the use of a table of random numbers. One restriction which was made in the assignment of the animals to the groups was that a proportional ratio of five females to eight males should be maintained for each group.

The pre-conditioning training of the animals, it will be seen, required considerable time. The habit of eating without the aid of the forelimbs appeared to be a difficult habit to learn. Furthermore, the restriction placed on the animals' gross bodily movements added to their adjustment difficulty. That the animals had made a satisfactory adjustment to the experimental situation is reflected in the fact that only two of the animals failed to learn the bar-flipping habit during the conditioning process. Both of these animals were females who exhibited excessive excitement and emotional behavior during the conditioning process.

2. *Conditioning.*—On the 20th day the series of reinforcing or conditioning trials was begun. On the first two trials a supplementary 'forcer' bar, $\frac{3}{4}$ in. closer to the floor of the platform than the standard bar, was used. When the platform was in the 'in-trials' position any slight movement of the *S*'s head hit this forcer bar, which activated the food mechanism and thus delivered a pellet of food to the cup. At the end of these two trials the forcer bar was removed, and on the subsequent trials of the experiment the standard bar, $1\frac{5}{8}$ in. above the floor of the platform, was used. The two trials in which the lowered bar was used were not considered as part of the acquisition trials.

Table I shows the various experimental conditions of the experiment and the number of animals in each group. During the acquisition training two conditions, I and II, are to be distinguished. In condition I the animal was given nine trials in which the bar-flipping response was reinforced. Following the consumption of the ninth food pellet, the animal was removed and

TABLE I

THE EXPERIMENTAL CONDITIONS FOR EACH GROUP AND THE NUMBER OF ANIMALS IN EACH GROUP

Experimental Condition	Number of Animals	Hours of Deprivation at Extinction	Temporal Inter-trial Period in Sec.
10 Reinforcements			
Group A_1	20	6	10
Group A_2	20	6	90
Group B_1	13	30	10
Group B_2	13	30	90
60 Reinforcements			
Group C_1	13	6	10
Group C_2	13	6	90
Group D_1	20	30	10
Group D_2	20	30	90

placed in a feeder where it was allowed to eat to the satiation criterion. In condition II the animal was given 59 reinforcements of the bar-flipping response after which it also was placed in a feeder and allowed to eat to the usual criterion. The temporal interval between trials during acquisition was varied according to a pre-arranged random order. The inter-trial periods in sec. were: 40, 50, 75, 65, 35, 55, 70, 45, and 60. Following the last (60-sec.) interval the temporal series was started again. This procedure was continued until the required number of reinforcements was given. It will be noticed that the mean number of sec. between trials for all animals was 49.5. If the remote possibility of temporal conditioning to the mean interval did occur, it will be noted that the points (10-sec. and 90-sec.) to which the response would be generalized during extinction would result in the responses at both intervals having equal strength. After the animals had reached the satiation criterion, the time was noted and they were returned to their home cage.

3. *Extinction.*—Groups A_1, A_2, C_1, and C_2 were brought back to the experimental situation six hours after they had reached the satiation criterion. They were placed in the apparatus and given one reinforced trial. Forty sec. following this trial the extinction series of non-reinforced trials was begun. Groups A_1 and C_1 were given the extinction trials with a temporal interval of 10 sec. between trials. Groups A_2 and C_2 were given the extinction trials with an inter-trial interval of 90 sec. An arbitrary criterion of extinction was used for all groups, the bar responses being considered extinguished when the latency of the response was 10 sec. or more for two successive trials. This interval was decided upon on the basis of data obtained from preliminary experimentation which suggested that all *S*s, even those receiving only 10 reinforcements, would reach a level of conditioning which would involve latencies shorter than 10 sec. Furthermore, a longer latency than 10 sec. presented the difficulty that spontaneous recovery might begin to complicate the results.

Groups B_1, B_2, D_1, and D_2 were brought back to the experimental situation 30 hours after they had reached the satiation criterion. They, too, were placed in the apparatus and given one reinforced trial. Forty sec. after this time the extinction series was started, and continued until the criterion was reached. Groups B_1 and D_1 had a ten-second inter-trial period between extinction trials. Groups B_2 and D_2 were given extinction trials separated by a 90-sec. interval. The experiment was concluded at the end of this session.

V. RESULTS

A. *Acquisition of the response.*—The acquisition curves, in terms of the latency of the conditioned response for the *S*s with 10 rein-

forcements and those with 60 reinforcements, are shown in Fig. 2. Each point on these curves represents the logarithm of the mean latency in sec. of the responses for 66 *Ss*.

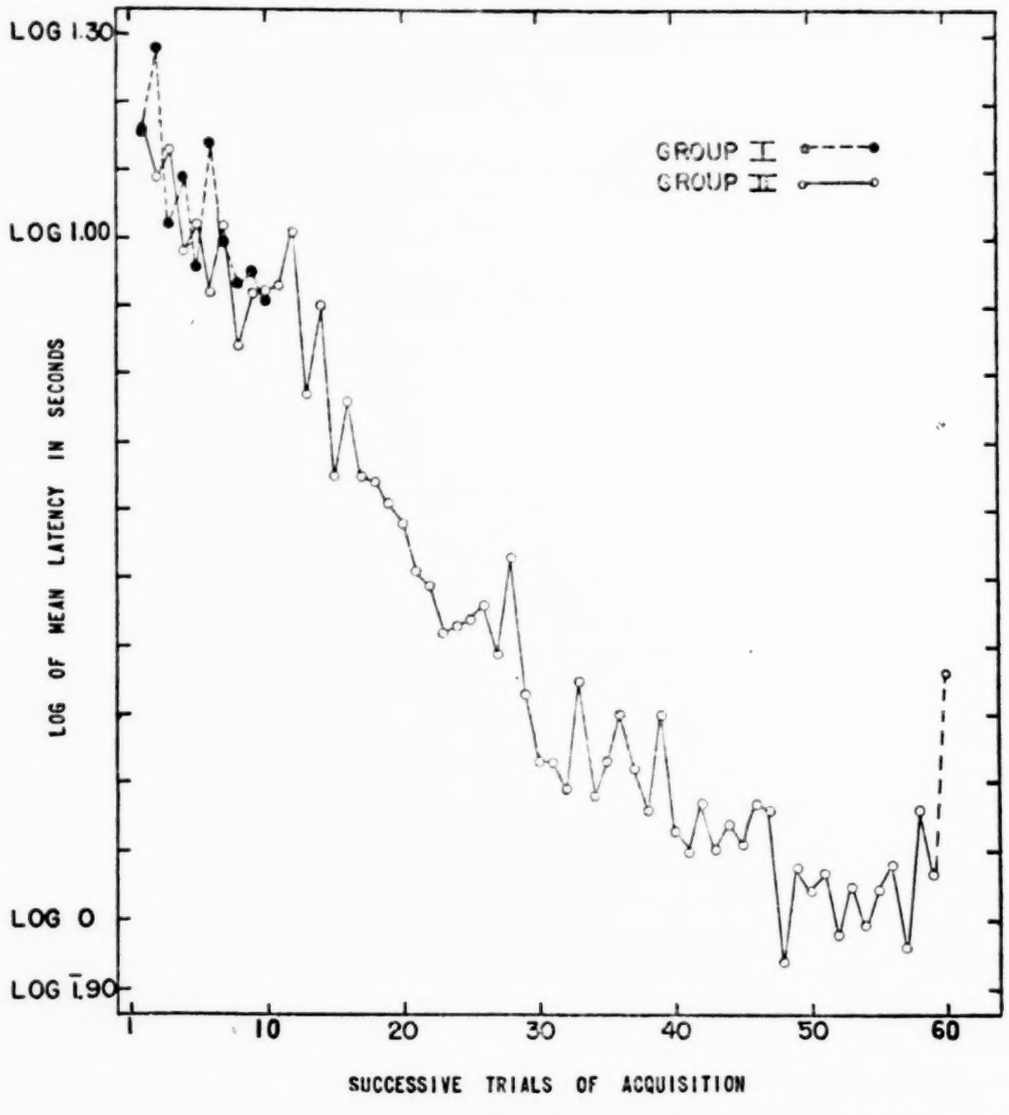

FIG. 2. Curves of acquisition of the bar response in terms of the latency of the conditioned response for the *Ss* in the 10-reinforcement and the 60-reinforcement groups. Each point on these curves represents the logarithm of the mean latency in sec. of the responses for 66 *Ss*.

In terms of this latency measure it will be seen that the two groups of *Ss* show about the same results for the first 10 trials. From an average response latency for the first trial, represented by the logarithm 1.1685, the curve drops to a value of 0.92942 at the tenth trial, which represents only about one-ninth of the distance to the asymptote of the curve or limit of response latency. The latter, it will be observed, appears to be reached at about 55 reinforcements. This compares with 66 trials to reach the limit as determined by Perin (17), who used resistance to extinction instead of latency in this

standard Skinner-type box, and with 75 trials (11) when Skinner's method of rate of response is used.

The curve represents the typical negatively accelerated form that characterizes such conditioning curves of latency measure. When one considers the fairly large number of *Ss* (66) on which the data are based, the curve shows a surprising variability from trial to trial. Curves plotted from the records of individual *Ss* show a marked tendency to such oscillation, but one would expect that with the combination of so many records this tendency would be almost completely smoothed out.

On the basis of these data it would appear that the present technique provides data essentially in agreement with those obtained by the usual Skinner box technique and other simple instrumental conditioning methods. The present arrangement has the advantage, however, that it provides for discrete trials and thus the measurement of response latency. It possesses the further advantage that it provides for more careful control of the animal's behavior during the learning period and, if desired, measures the amount of ongoing concomitant activity.

The chief disadvantage of the method would appear to be the great expenditure of time and effort on the part of the experimenter that it involves. The *Ss* must first be trained to eat in the holder and then, following this, to be adapted to the conditioning apparatus. Unless it is desirable to obtain a record of the response latency and the concomitant behavioral changes, the standard Skinner-box technique is much more efficient.

B. *Withdrawal responses made during acquisition.*—The term 'withdrawal response' refers in the present study to those response patterns which resulted in the backward movement of the stock which closed the switch and activated the polygraph. For the most part the patterns of behavior involved were apparent attempts to withdraw from the stock and general struggling.

The measure upon which the analysis of the withdrawal response is based was obtained by counting the number of times the marker was activated by the closing of the switch during each trial period. The particular type of stock used in the main body of the experiment was not incorporated into the apparatus until after the first 16 *Ss* had finished the experiment. The analysis of the withdrawal responses therefore does not include these first *Ss* which were distributed evenly over all groups.

Fig. 3 shows the changes in withdrawal responses that took place during the original learning. The data for all the 10-reinforcement *Ss* and all the 60-reinforcement *Ss* were combined, and Vincent curves in terms of tenths of the total acquisition period were determined for each group. The upper broken curve thus shows the changes that took place over the first 10 trials for the *Ss* that were given only this number of reinforcements, while the lower solid curve represents the

change over the longer period of 60 training trials. Each point on this curve thus represents the data for six trials.

It will be seen that with greater amounts of training on the bar-flipping response the frequency of withdrawal responses became fewer and fewer. This decrease presumably represents the joint

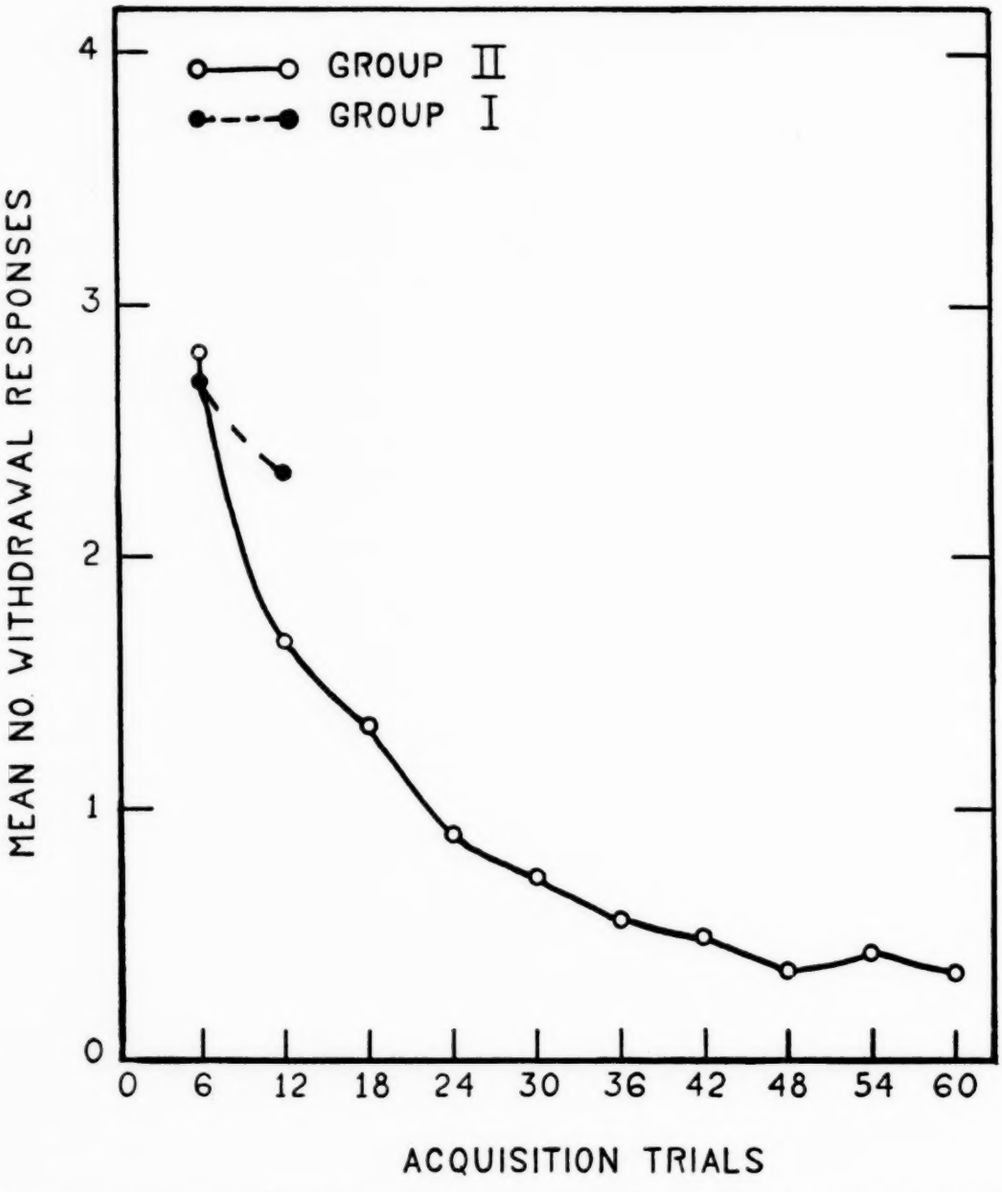

FIG. 3. Vincent curves showing the changes in frequency during acquisition training of the withdrawal responses made by the animals in the 10-reinforcement and 60-reinforcement groups. Each experimental point represents successive tenths of the total acquisition period.

action of negative adaptation of the struggling behavior and the increasing strength with reinforcement of the response to the bar. The latter act was quite in conflict with the withdrawal response, requiring the *S* to press forward and upward in order to reach the bar.

Hilgard and Marquis (11) have referred to a quite similar phenomenon which they have termed *counter conditioning*. Counter conditioning is said

to take place when the original response made to the stimulus situation is incompatible with the new response being learned and the old response is displaced by the new.

The present instance of this phenomenon is strikingly similar to one of the examples of counter conditioning cited by Hilgard and Marquis (11, p. 113). They refer to an experiment with dogs carried out by one of Pavlov's co-workers in which an electric shock was employed as a conditioned stimulus with the presentation of food as the unconditioned stimulus. The struggling and barking originally evoked by the shock was not compatible with the motor responses made to the food stimulus with the result that the original response pattern gradually disappeared.

C. *Experimental extinction as a function of the temporal interval between trials.*—The experimental results bearing on the effect of massing and distribution of the extinction trials are shown in Table II. It will be recalled that the two different inter-trial intervals, 10 and 90 sec., were employed with four different conditions of reinforcement and motivation. We shall refer henceforth to the 10-sec. groups as the massed groups, and to the 90-sec. groups as the distributed groups.

TABLE II

THE MEAN AND MEDIAN NUMBER OF TRIALS REQUIRED TO REACH THE EXTINCTION CRITERION BY THE VARIED EXPERIMENTAL GROUPS

Experimental Group	*N*	Mean	Standard Error of Mean	Median	Standard Error of Median
A_1 (massed)	20	16.10	3.573	11.00	4.478
A_2 (dist.)	20	10.05	2.015	8.00	2.525
B_1 (massed)	13	26.92	4.997	20.55	6.263
B_2 (dist.)	13	24.30	4.114	28.25	5.156
C_1 (massed)	13	25.54	4.507	25.50	5.648
C_2 (dist.)	13	32.55	4.427	37.50	5.548
D_1 (massed)	20	37.70	4.436	32.50	5.559
D_2 (dist.)	20	49.70	4.595	54.50	5.759

Table II gives the median and mean number of trials required by the animals in each of the eight groups to reach the extinction criterion, together with their standard errors. It is to be noticed first of all that the groups which had received 60 reinforcements in the original conditioning required a larger number of trials to extinguish the response than the groups which were given 10 original reinforcements. The differences are not great, however, between the highly motivated, low reinforcement groups (B_1 and B_2) and the lowly motivated high reinforcement groups (C_1 and C_2). On the whole, the differences would seem to support the inference that the high reinforcement groups showed the greater amount of resistance to extinction. The low reinforcement, low motivation groups definitely

showed the least resistance to extinction and the high reinforcement, high motivation groups the greatest resistance to extinction.

When we turn to a comparison of the massed and distributed groups we find what at first appears to be a somewhat conflicting picture. Tables III and IV show the magnitude and direction of the

TABLE III

ANALYSIS OF THE DIFFERENCES BETWEEN THE MEAN NUMBERS OF TRIALS REQUIRED TO REACH CRITERION BY THE VARIOUS GROUPS

Comparison between Means of Groups	Difference	Difference in Favor of t*	Significant at
A_2—A_1	−6.05	90 sec. group 2.0934	2 to 5% level
B_2—B_1	−2.62	90 sec. group 0.5749	60 to 50% level
C_2—C_1	+7.01	10 sec. group 1.5600	10 to 20% level
D_2—D_1	+12.00	10 sec. group 2.6386	1% level

* Values of t based upon error term from analysis of variance

TABLE IV

ANALYSIS OF THE DIFFERENCE BETWEEN THE MEDIAN NUMBERS OF TRIALS REQUIRED TO REACH CRITERION BY THE VARIOUS GROUPS

Comparison between Medians of Groups	Difference	Difference in Favor of	Critical Ratio	Significant at
A_2—A_1	−3.0	90 sec. group	0.5951	28% level
B_2—B_1	+7.5	10 sec. group	0.9808	16% level
C_2—C_1	+12.0	10 sec. group	1.516	6% level
D_2—D_1	+22.0	10 sec. group	2.749	0.3% level

differences between these groups, i.e., whether in favor of the 10- or 90-sec. groups in speed of extinction, and also to what degree the differences are significant. From Table III which presents this information for the means, we see that the extinction process was apparently facilitated by distribution of the trials in those groups which had only 10 reinforcements, whereas in the case of the groups with 60 reinforcements the massing condition led to the more rapid extinction. Two of the differences are significant at the five percent level or better.

The most significant difference is that between the two groups (D_1 and D_2) which had received 60 reinforcements and had been without food for 30 hours at the time of extinction. In this instance the massed group required 12 less trials on the average than the distributed groups. The other difference that attained an acceptable degree of statistical significance points in the opposite direction. It was attained in the case of the subjects (A_1 and A_2) which had only 10 reinforcements and were run under six hours food deprivation.

The difference in this instance favored the distributed group and was found to be significant at the five percent level.

The difference between the median measures presents a somewhat different picture. Thus Table IV shows that three of the four differences, those between B_1 and B_2, C_1 and C_2, and D_1 and D_2, favor the massed condition. The latter two, it will be observed, were statistically significant. While the difference between A_1 and A_2 groups is again in favor of the distributed group it is no longer statistically significant, the critical ratio being only 0.8285. The data of this table suggest the possibility that the difference at low response strength (few reinforcements and low motivation) is zero and that as the strength of the conditioned response increases the difference in speed of extinction between the massed and the distributed condition increases.

Support for such a conclusion is to be found in the results of a further analysis of the data for the A groups. It will be noted that in the present experiment the usual method of conditioning all *S*s until they reach the same criterion level of performance (e.g., response latency) was not employed, but instead all the *S*s within a particular group were given the same number of reinforcement trials. The assumption was made in this case that number of reinforcements was a reliable index of the strength of response at the time the extinction process was begun. If, however, we assume that mean response latency is a reliable index of the strength of response at the time the extinction process was begun and make an analysis based upon this assumption, the conclusion arrived at might be entirely at variance with the conclusion based on the index, number of reinforcements. Thus, when such a check was instituted in terms of the mean response latencies for training trials seven, eight, and nine for all the animals used in the experiment, it was found that there was a statistically significant difference at the two to five percent level between these measures for the groups, A_1 and A_2. The group which was extinguished under distributed conditions had a mean latency per trial for these three trials of 28.46 sec. as compared with a mean latency of 12.22 sec. for the *S*s which were subsequently extinguished under massed conditions, indicating that the response strength of the A_2 groups as measured by the latency criterion was much greater than the response strength of the A_1 group *at the time extinction was started.* That such a difference is an important factor to be controlled is shown by the fact that fairly high correlations were found between these response latency measures and measures of resistance to extinction for both groups. Thus, in groups A_1 the correlation coefficient (rho) was .70, while in the A_2 group it was .46. Unfortunately, differences in the same direction occurred in each of the three other pairs of groups (B, C, and D). However, they were not statistically significant, although they were of considerable magnitude in the case of the D groups.

The results of this analysis indicate, then, that the differences in favor of the massed condition for the high reinforcement groups occurred in spite

of this failure to obtain equality in the level of original learning. In an attempt to ascertain the effect of removing or controlling this factor, as many *Ss* as possible of the A_1 and A_2 groups were matched with each other on the basis of their response latencies in the last three trials of the first conditioning period. The results for 10 such paired *Ss* showed on the subsequent extinction trials a difference of 3.2 responses between the means in favor of the distributed group, as compared with a difference of 6.05 in the case of the random groups. Neither of these differences were statistically significant.

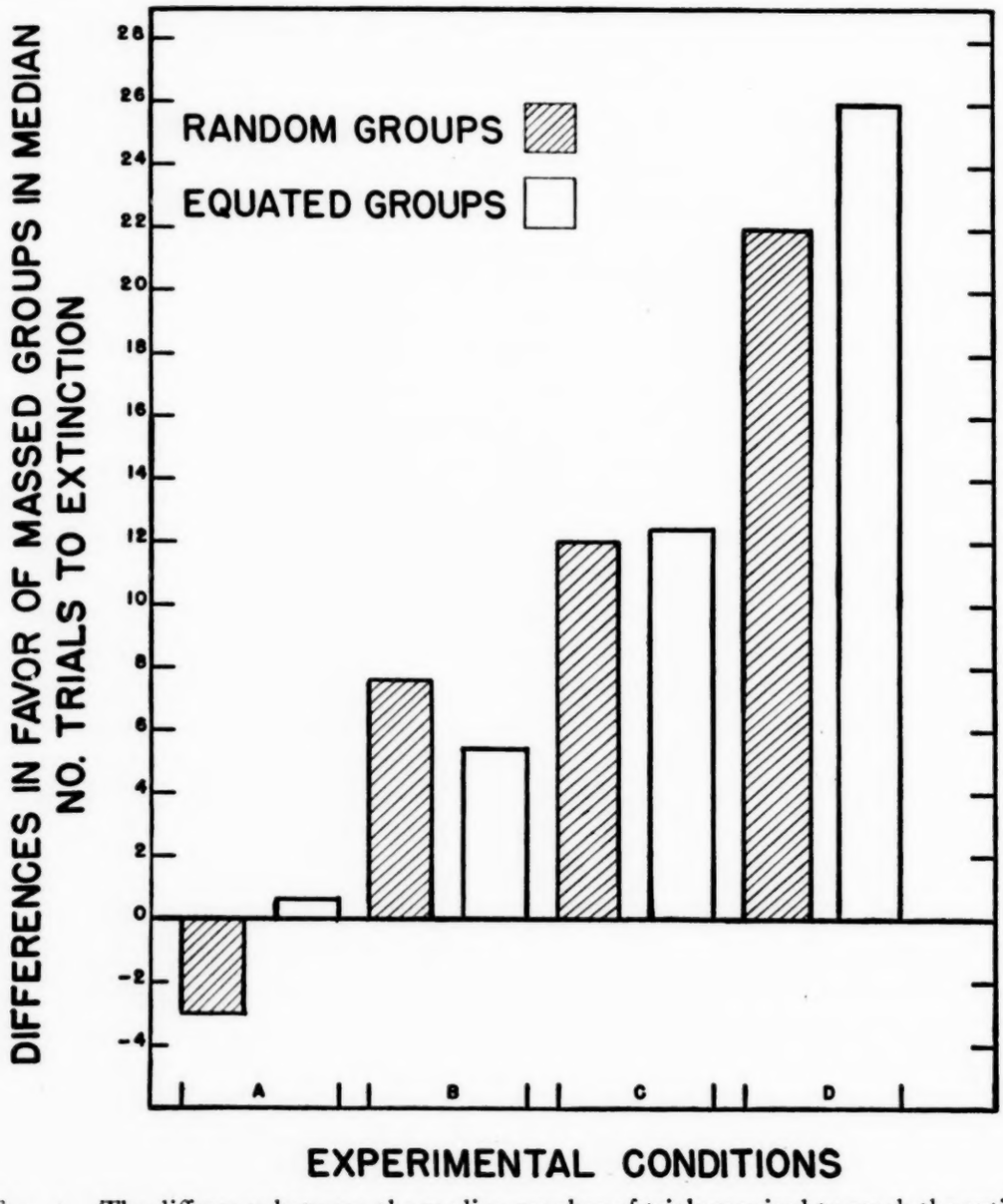

FIG. 4. The difference between the median number of trials required to reach the extinction criterion by the temporally massed and distributed groups at each of the reinforcement-motivation conditions. The comparison is made for the original randomized groups and for the selected matched groups.

Fig. 4 attempts to summarize the results of the experiment so far as the influence of massing and distribution of the extinction trials is concerned. The column bars indicate the direction and magnitude

of the differences between the median number of trials to reach extinction for the massed and distributed groups at each of the reinforcement-motivation conditions. The filled bars represent the difference between the medians as taken from Table III. The solid bars are measures of the difference between the medians which resulted when the groups were first matched on the basis of latency scores by the method described above in the case of A groups. Both of these sets of measures show the same general trend, an increasing facilitation of extinction under massing as compared with the distribution, the higher the reinforcement-motivation level. The differences for both sets of 60 reinforcement groups are statistically significant.

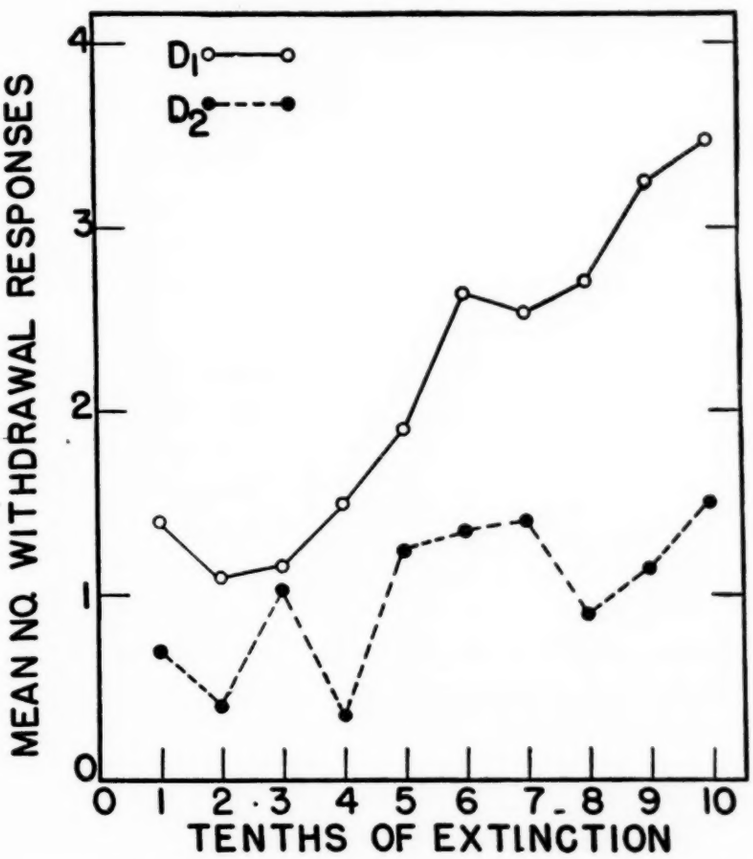

FIG. 5. Vincent curves based upon the frequency of occurrence of the withdrawal responses for successive tenths of the extinction period. High reinforcement-high motivation groups.

D. *Analysis of withdrawal responses made during extinction.*—As previously pointed out the term 'withdrawal response' refers in the present discussion to those response patterns which resulted in the backward movement of the stock which closed a switch and activated the marker on a polygraph.

With failure of reinforcement of the bar-flipping response during the extinction series and its consequent weakening, the withdrawal response tends to return. This is shown very clearly in Fig. 5, which presents curves showing the mean number of withdrawal responses

per trial that occurred throughout successive tenths of the extinction period for the groups D_1 and D_2. While the tendency for these responses to return is more marked and persistent for these groups, particularly the *Ss* extinguished under massed conditions (group D_1), all the groups showed a tendency for the withdrawal responses to increase in number during extinction.

TABLE V

AN ANALYSIS OF THE SIGNIFICANCE OF THE DIFFERENCE BETWEEN THE MEAN NUMBERS OF WITHDRAWAL RESPONSES MADE BY THE VARIED GROUPS DURING THE FIRST EXTINCTION PERIOD. ALL OF THE EXPERIMENTAL VARIABLES WERE HELD CONSTANT EXCEPT THE VARIABLE, TEMPORAL PERIOD BETWEEN NON-REINFORCED TRIALS.

Group	*N*	Mean Number of Withdrawal Responses	Diff. in Favor of	Diff. Value	*t*	Significant at
A_1	18	14.66	A_1	1.889	0.615	60–50% level
A_2	18	12.77				
B_1	11	14.727	B_1	2.182	0.675	50% level
B_2	11	12.545				
C_1	11	19.727	C_1	6.454	1.225	20–30% level
C_2	11	13.273				
D_1	18	21.888	D_1	12.444	2.379	2–1% level
D_2	18	9.444				

Table V presents the data on the withdrawal responses for all the extinction groups. The data are presented in terms of the mean number of withdrawal responses made by the eight different groups during the extinction period, the differences between the massed and distributed groups for each level of motivation, and the statistical significance of those differences. The result of the analysis of these data indicates that there is a general trend for the massed groups to make more withdrawal responses than do the distributed groups, the trend culminating in a difference between the D groups which is significant at between the two and one percent level. It is interesting to note that this observed trend of more withdrawal responses with massed extinction trials parallels the trend noted in the corrected group for speed of extinction; namely, the more massed the trials the faster the rate of extinction.

VI. Discussion of Results

Our main interest in varying the temporal period between non-reinforced trials during the process of extinguishing a conditioned

response lies in the implication which the resulting data have for the two theories which attempt to describe the mechanism underlying the response decrement that occurs: the internal inhibition and the interference theories. Pavlov, it will be recalled, accounted for extinction in terms of his hypothetical concept, internal inhibition. Neglecting its supposed neurophysiological characteristics, which are unessential to a behavioral theory, internal inhibition was assumed by Pavlov to be built up as a result of presenting the conditioned stimulus without the reinforcing unconditioned stimulus. This state of inhibition was postulated as acting in opposition to the excitatory state, another hypothetical concept of Pavlov's, which was assumed to develop as the result of the original conditioning or reinforcement. These two processes were thought of as interacting in a kind of algebraic manner to determine at any trial the effective strength of the tendency for the conditioned stimulus to evoke the response. Pavlov made the further postulate that whereas the excitatory state was fairly stable and more or less permanent in nature, the inhibitory state was a labile, fragile process which dissipated at least a portion of itself fairly promptly with the passage of time. It is this last assumption about inhibition that leads to the implication that the progress of experimental extinction should be facilitated by the massing of trials. For it follows from this assumption that longer intervals between trials would provide more time for the dissipation of the inhibition with the result that it would cumulate more slowly per trial and thus require a larger number of trials to obtain a sufficient amount of inhibition to offset the excitation developed as the result of conditioning. Under conditions of massing, the inter-trial intervals would be short and relatively little inhibition would be dissipated between each trial. Consequently extinction should occur more rapidly under the massed condition.

The results of the present experiment tend strongly to support some such interpretation as Pavlov offers. They are, moreover, in disagreement with the interference type of theory. The latter, it will be recalled, denies the necessity of postulating (as Pavlov does) the additional concept of internal inhibition. A single concept of the excitation type is sufficient according to the interference theorists. The decrement or disappearance of the conditioned response in extinction is explained as being due to interference from a competing response which in turn becomes conditioned to the stimulus. For the interference theorists, the extinction of a response really involves the conditioning of a new, competing response which interferes with the old; extinction and conditioning are just two aspects of the same process. If we observe and record the old response which is interfered with and disappears we speak of extinction; when our concern

is with the new response we refer to conditioning. It follows that, being one and the same process, conditioning and extinction should be effected in identical ways, by massing and distribution of trials. The experimental results concerning the effects of this variable on conditioning are fairly unequivocal; massing has consistently led to retardation of conditioning. The implication of this theory that extinction should likewise be retarded receives little support from the present study.

Attention should be called here to the fact that while some studies have not agreed with the results of the present experiment in showing that massing facilitated extinction, there have been no experiments in which distribution has led to quicker extinction as would be predicted from an interference type theory. Either no difference has been found between the conditions of massing and distribution or a difference was found in favor of massing which was not statistically significant.

Returning to the Pavlovian theory, it should be pointed out that there is one part of it that is not supported by the present experimental data in the manner that Pavlov described. In addition to the postulates outlined above, Pavlov made the further assumption that the inhibitory stage generated by extinction would irradiate and result in a general decrement in all responses of the *S* to the situation. Pavlov reported that in his experiments, which involved severe restrictions of the animal, this irradiation of inhibition was so pronounced that his *S*s showed little or no activity and often went to sleep during extinction.

Other investigators, however, have failed to find evidence for such widespread depression of behavior as a consequence of extinction. Thus, Wendt (**26**) and Kantrow (**14**) reported qualitative observations which indicated that the activity level increased as extinction proceeded. The analysis of the withdrawal responses during extinction in the present experiment likewise failed to support Pavlov's irradiation theory and experimental findings, for it will be recalled that measurement of the withdrawal activity showed a marked increase during the course of extinction. These findings, however, have little or no bearing on the main core of Pavlov's theory of internal inhibition and they do not necessarily refute it as Wendt (**27**) seems to have reasoned. More properly they should be interpreted as calling into question only that portion of the theory which involves the postulate of irradiation of inhibition.

A theory with which the data of the present experiment are in agreement and which represents somewhat of a compromise between the internal inhibition theory of Pavlov and the interference type of theory has recently been proposed by Hull (**13**). His theory is a kind of two factor theory, and in this respect is similar to Culler's

theory (2). One component, designated by Hull as reactive inhibition (I_R), is supposed to develop as the result of the active occurrence of the response itself. This concept is defined at a motivating state. It is assumed to accumulate from trial to trial and to disappear in the intervals between trials. With massing of trials during conditioning more of this I_R accumulates than in distributed training, with the result that conditioning is slowed down. The only reason that positive conditioning even takes place under conditions of massing of trials, according to this formulation, is that the increments of habit strength resulting from a reinforcement more than compensate for those increments of inhibition. In extinction, however, there are no such incremental effects from reinforcement with the result that a decrement in response occurs. Massing of extinction trials leads to quicker response decrement because less time is permitted between trials to recover from the work or fatigue inhibition (I_R). This component of Hull's theory, while thought of as being something quite different from Pavlov's concept of internal inhibition so far as its physiological characteristics are concerned, is similar to it in its behavioral properties. That is, it is assumed to disappear with the passage of time and to accumulate with positive stimulation.

The second component of Hull's theory is a learned or habit phenomenon, and in this respect his theory is like the interference theories in that the assumption is made that a form of competing response is acquired during extinction. Beginning with the assumption that the fatigue-like condition of I_R constitutes a motivating state, a need for rest, Hull employs other aspects of his general theory of learning to the effect that cessation of the activity which generated the need would initiate a reduction in the rest-need level and thus provide reinforcement of this response of ceasing the act. The response of ceasing or stopping the act would thus become conditioned to the stimulus situation. Hull terms this component conditioned inhibition (${}_sI_R$). It consists essentially of a negative habit, i.e., the response of not responding. The two components (${}_sI_R$ and I_R) summate together to produce the total effective inhibition ($\dot{I}_R$).

If one conceives of Hull's concept, ${}_sI_R$, as being the habit strength of any act which is in direct competition with the performance of the conditioned response being extinguished, rather than the more limited cessation of an act, then the withdrawal data obtained in the present experiment would supply an empirical example of the manner in which the varying strength of the need condition, I_R, governs the speed with which the phenomenon of extinction takes place.[4]

[4] The recurrence of withdrawal responses, during the extinction series in this study, has an interesting relationship to the literature on habit regression, reviewed by R. R. Sears in *Survey of objective studies of psychoanalytic phenomena*, N. Y.: Soc. Sci. Res. Council, 1943. A discussion of that relationship is, however, beyond the scope of this paper.

Summary

1. The rate of development of experimental extinction of an instrumental conditioned response as a function of the length of the interval between extinction trials, and the strength of the response tendency undergoing extinction, were investigated.

2. A total of 132 albino rats were first trained in a modified Skinner apparatus which required the *S* to raise a horizontal bar by an upward movement of its head in order to obtain food-reward. Extinction trials were then given under massed or distributed conditions at four different combinations of the two variables determinng the strength of the response tendency: (a) the number of original training trials; and (b) the hours of food deprivation at the time of extinction.

3. The results comparing speed of extinction under massed and distributed conditions were shown to favor the massed condition when the response being extinguished was one with high habit strength. There was evidence that the differences between the massed and distributed conditions increased with the strength of the response tendency undergoing extinction. The differences were statistically significant for groups C and D.

4. The extinction data resulting from the groups which had few original reinforcements offer no conclusive evidence in support of the efficacy of either massing or the distribution of extinction trials. The inconclusive nature of these data apparently was a result of the failure to equate the low reinforcement groups on the basis of the strength of original conditioning.

5. An analysis of the withdrawal responses made by the animals showed that during the acquisition trials there was a gradual decrease in the number of withdrawal responses made, whereas during the extinction period the number of withdrawal responses increased with a decrease in the strength of the bar response. Moreover, the animals which were extinguished under massed conditions exhibited a greater number of withdrawal responses than the animals extinguished under the distributed conditions. The latter differences were statistically significant for the high reinforcement-high motivation group.

The results were interpreted as contrary to the implications of the interference type of theory of extinction phenomena and as supporting the Pavlovian theory of internal inhibition except for that portion which assumes that the inhibition irradiates to other acts producing a generalized state of depression of activity.

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A POSITIVE RELATIONSHIP BETWEEN REINFORCEMENT AND RESISTANCE TO EXTINCTION PRODUCED BY REMOVING A SOURCE OF CONFUSION FROM A TECHNIQUE THAT HAD PRODUCED OPPOSITE RESULTS

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Introduction

One of the propositions in behavior theory (**10, 12**) is that, when other conditions are held constant, the number of non-reinforced trials required to extinguish a given response is directly related to the number of reinforced trials that response has received during training. This proposition has been supported by a number of experimental studies (**11, 16, 18, 20**). To date, the main qualification on this principle has been the fact that the relationship is a negatively accelerated one, or, in other words, proceeds with diminishing returns, so that eventually a point is reached where further increases in the number of reinforced trials will produce little if any increase in the resistance to extinction. A recent study by Finger (**2**), however, reports results which appear to contradict this proposition by showing an *inverse* relationship between the number of reinforcements and resistance to extinction. Under the conditions of his experiment, animals that had received 16 reinforcements reached a criterion of extinction in fewer trials than those that had received only eight reinforcements. This finding was confirmed in part by two other studies employing the same general procedure and apparatus (**3, 13**).

In view of the theoretical importance of the problem, it seems desirable to see if Finger's results mean that there is no lawful relationship between the number of reinforcements and resistance to extinction or if, on the other hand, they were a function of some aspect of his experimental technique that introduced a disturbing factor counteracting the effects of reinforcement. The fact that Finger's technique has been used in a number of coordinated studies (**2, 3, 5, 6, 7, 8, 13, 14, 15**) designed to investigate the variables influencing learning, increases the desirability of determining whether or not it involved such a source of confusion. That the technique may have contained some peculiar factor of this kind is suggested by a second paradoxical result found in the above studies: namely, that animals

given 16 training trials showed so much decrement when tested for forgetting that their retention of the habit was no better than that of animals given only eight trials (3, 15).

In Finger's experiment and also in the other experiments in that series, the response measured was the animal's latency in leaving the starting box for a run down an elevated runway three feet long and $1\frac{1}{4}$ in. wide. The animals were motivated by hunger and reinforced by food. In an attempt to minimize handling of the animals, the construction of the starting box and the goal box was identical. After the animal had eaten the food in the goal box, the box with the animal in it was carried around to the beginning of the maze and used as the starting box, while the erstwhile starting box was used at the goal. In examining Finger's procedure it seemed likely that this identity of the starting and goal boxes and a lack of other distinctive cues, such as those for spatial orientation, was the source of the disturbing factor obscuring the effect of number of reinforcements[1]. In the absence of distinctive cues differentiating the two boxes, it seemed possible that during the rewarded trials the animals were learning some habit in the goal box (such as looking for food) which interfered with their leaving it when it was supposed to be functioning as the starting box. The differential strengthening of the conflicting habits during the latter part of training might have obscured the effects of additional reinforcements.

The purpose of the present experiment is to determine whether or not the paradoxical inverse relationship between reinforcement and resistance to extinction will still appear if distinctive cues are introduced to remove the identity of the starting and goal boxes as a possible source of conflicting responses. This will narrow down the problem. As will be shown in the discussion, the details of the possible ways in which the lack of distinctive cues could function in this type of an experimental situation turn out to be fairly complex.

In the present experiment the distinctive cues, differentiating the start from the goal, were introduced by using physically different starting and goal boxes, by permitting the animal greater spatial orientation, by handling the animals at the beginning and end of each trial, and by restraining them between trials in a special cage.

In addition to this change in procedure, two levels of reward were used. This variable was introduced in part to determine the relationship between the amount of reward and the strength of a habit, and in part to determine if the small amount of reward used by Finger contributed to his anomalous results. Finger used one pellet of Purina dog chow, weighing .04g m., per trial. Half of our animals

[1] A similar hypothesis has been suggested by Mote (13) to account for a leveling off of the learning curve. He has not applied this hypothesis to the process of extinction.

received the same amount and the other half received four times as much.

In all other essential characteristics the apparatus and procedure were similar to those used in Finger's experiment.

Apparatus

The general plan of our apparatus was based on the one used by Finger (2). The main points of difference between the two were the change in the construction of the goal box and the lack of a oneway screen around our apparatus. The runway was three feet long and 1¼ in. wide and was three feet from the floor. The starting box had inside dimensions of 5½ by 6 by 7 in. with a solid hinged top for inserting the animal. The front of this box was covered by a wooden screen 16 by 17 in. extending two in. below the level of the runway. The screen contained a sliding door that could be raised manually. Both the screen and the box were painted black.

The goal consisted of a white platform 5¼ by 7 in., backed by a white wooden board 8 by 9 in. To the latter was attached a small brass food cup, deep enough so that the pellet of food was not visible except from above. The sides of the platform were open to the room. A 60-watt desk lamp was placed so as to shine directly on the goal platform.

The runway was covered by fine copper screening except for a ⅛ in. gap eight in. from the starting box. The leads of an electronic relay led to the sides of the gap. When an animal stepped across the gap, it completed a circuit operating a relay. The flow of current was subliminal and offered no barrier to the running behavior of the animal.

Procedure and Animals

The animals were 32 hooded male rats approximately 90 days old at the start of the experiment. They had been tamed by handling for several days previous to the experiment. Eight of these animals were discarded during the adaptation training on the basis of the selection procedure described below.

Our adaptation procedure paralleled that used by Finger. Four adaptation trials preceded the experiment proper. The animals were 24 hours hungry at the time of the first session. They were placed on a wooden runway (eight ft. by one in.) for 10 min. and then fed for 20 min. on a platform similar to the goal platform but not attached to the apparatus. This was repeated on four successive days. During this fourth adaptation session, the animals to be included in the experiment were selected on the basis of their behavior on the practice runway. This selection procedure was adopted because preliminary observations indicated that some animals were reluctant to move about on such a narrow path. In practice, six pellets of food were distributed along the runway. If an animal did not run from one to the other and eat them all during the 10 min. adaptation period, he was discarded. In this way, eight out of the original 32 animals were eliminated.

At the end of adaptation training the 24 experimental animals were randomly distributed between the four groups, six animals to a group, with the following reinforcement schedule during acquisition trials.

Group I. (8–1) 8 reinforced trials with one pellet of food (.04 gm.) on each trial
Group II. (8–4) 8 reinforced trials with 4 pellets per trial
Group III. (16–1) 16 reinforced trials with 1 pellet per trial
Group IV. (16–4) 16 reinforced trials with 4 pellets per trial

This was done in order to determine the influence of both number of trials and amount of reward on resistance to extinction.

The experiment proper was begun on the fifth day. The animal was placed in the starting box. Ten sec. later the door was raised and an electric clock started. When the animal stepped across the gap, eight in. from the door, the electronic relay stopped the clock. In this way a measure of 'latency' was recorded. If the animal retraced, the clock was reactivated so that the measure of latency was actually a measure of the time spent at the starting end of the runway. (This is the same procedure that was used to measure latency in Finger's experiment.) When the animal had eaten the food in the food dish, he was picked up and placed in a restraining cage for 45 sec. He was then returned to the starting box and the next trial started. Thus, approxi-

mately 75 sec. elapsed between trials. Reinforcement during acquisition trials consisted of pellets of purina dog chow weighing approximately .04 gm. The animals had previously been fed on these during adaptation training.

The extinction trials followed the reinforced trials without interruption. The only change from the learning procedure was the omission of food pellets. During extinction, a trial was considered to have been completed if an animal placed three of his feet on the goal platform even though he did not approach the food cup. This was necessary in that during extinction an animal would often explore about the goal platform without approaching the food cup and then attempt to retrace. Extinction trials were continued until an animal obtained latencies of five minutes on two successive trials. At the end of the extinction trials all animals were placed in their home cages after having been kept for approximately five min. in the restraining cage. Groups I, II and III were immediately fed a sufficient number of additional pellets so that their day's total would equal the 64 obtained by Group IV during the learning trials. Twenty-four hours later, they were re-run for eight reinforced trials, all groups receiving one pellet per trial.

Results

If the number of reinforced trials is positively related to the strength of a habit, then the following relations should be found in this study. As compared to the 8-reinforcement group, the 16-reinforcement group should:

1. Require on the average more trials to reach a criterion of extinction,
2. Have a smaller mean latency during the first few trials of extinction before any of the animals have reached the criterion,
3. Show more spontaneous recovery after 24 hours,
4. Have a smaller mean latency during a set number of relearning trials.

The same relationships should hold in comparing the four-pellet group with the one-pellet group. In addition the four-pellet group should learn more rapidly than the one-pellet group during the original learning trials.

To determine if the first point above held for the present data, an analysis of variance was done on the number of trials required by the various groups to reach the criterion of extinction. The relevance of the other three points was determined by fitting curves to the latency scores for each animal and then undertaking an analysis of variance of these performance curves. The results are discussed in the following paragraphs.

1. *Resistance to extinction.*—This was the measure used by Finger in securing his paradoxical results, and hence the most pertinent one for the main purpose of the present study.

a. Number of reinforced trials.—The primary concern of this experiment was to determine the relationship between the number of reinforced trials and the number of extinction trials required to reach a fixed criterion of extinction. This relationship is shown in Table I,

TABLE I

ANALYSIS OF THE NUMBER OF TRIALS REQUIRED TO REACH THE CRITERIA OF EXTINCTION

Analysis of Variance				Means and Variances				
	d.f.	3 Min.	5 Min.	Group	3 Min.		5 Min.	
		Variance	Variance		Mean	Variance	Mean	Variance
Reinforcements	1	513	840	I	10.2	18.2	13.3	17.8
Pellets	1	198	38	II	10.7	59.4	13.0	41.6
Interaction	1	165	48	III	14.2	98.2	22.3	183.6
Error	20	63	88	IV	25.2	75.4	27.7	109.5

$$F_1 = \frac{513}{63} = 8.14^{**} \quad F_2 = \frac{198}{63} = 3.14 \quad F_3 = \frac{840}{88} = 9.55^{**} \quad F_4 = \frac{38}{88} = .43$$

** Indicates significance at one percent level of confidence.

which gives the mean number of trials and variance required by each group to obtain the first three min. latency and also to obtain two successive five min. latencies. The 16-reinforcement groups combined averaged 19.7 trials to the three min. criterion (the one used by Finger) and 25.0 trials to the five min. criterion, whereas the 8-reinforcement groups combined required 10.5 and 13.2 trials. Thus, in this experiment, there is a direct relationship between the number of reinforcements and resistance to extinction. This finding is directly opposed to that of Finger, who found a tendency for the 16-reinforcement group to extinguish more rapidly than the 8-reinforcement group.

The reliability of the above differences was determined by an analysis of variance of the data in Table I. This seemed justified in that Bartlett's test for homogeneity of variances (**17**) indicated that the level of significance for the chi-square was only 30 percent for the three min. criterion and seven percent for the five min. one. The results of the analysis are shown in Table I (F_1 and F_3). At each criterion there is less than one chance in 100 that as large or larger differences between the means of the 8- and 16-reinforcement groups would result from random sampling alone.

b. Amount of reward.—Table I indicates that the four-pellet groups combined took on the average 17.9 trials to reach the three min. criterion and 20.3 to reach the five min. criterion, whereas the one-pellet groups combined took 12.2 and 17.8. The differences are in the expected direction but are relatively small. The analysis of variance in Table I (F_2 and F_4) indicates that neither the difference at the three nor at the five min. criterion is significant at the five percent level of confidence. Consequently, while there is some

indication of the expected relationship, the null hypothesis cannot be reliably rejected.

2. *Analysis of performance curves.*—In testing whether the other measures of the strength of a habit supported the finding on resistance to extinction, an analysis of variance of individual performance curves was done for the learning, extinction and relearning trials. All the latencies, measured to the nearest tenth of a sec., were converted to reciprocals[2] and multiplied by 1000. Consequently the transformed latencies are referred to as 'speeds.'

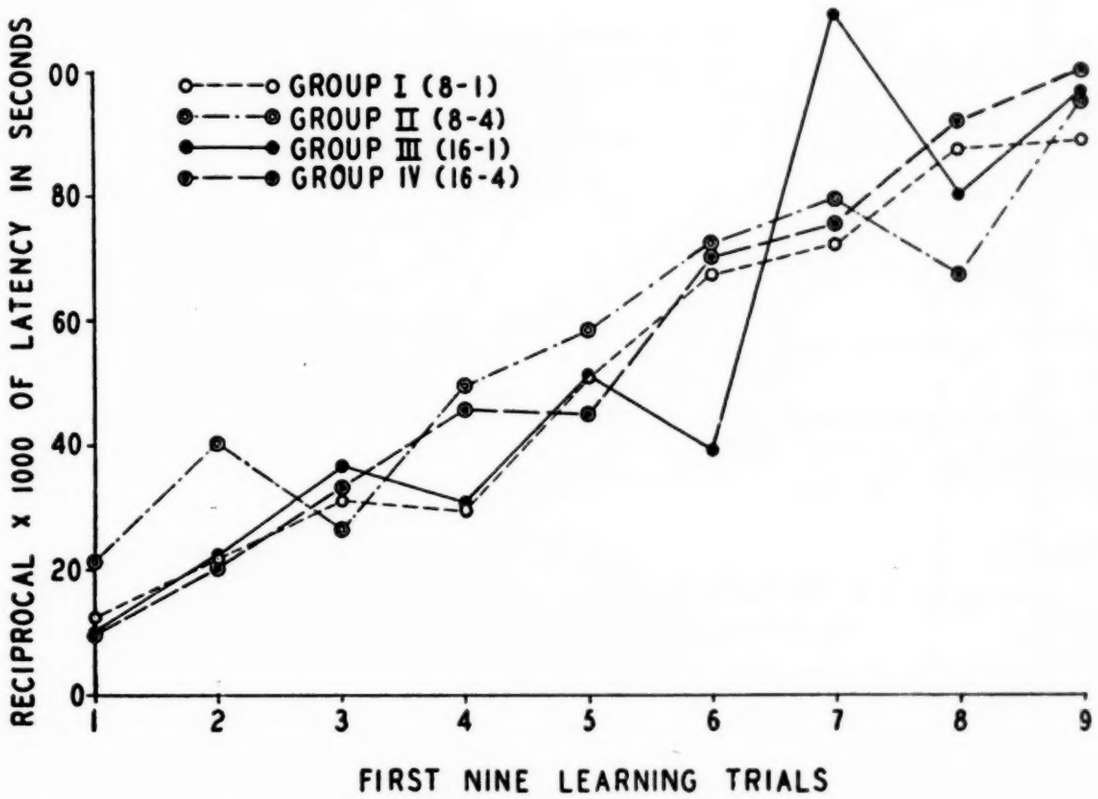

FIG. 1. First nine learning trials. All four groups learned at approximately the same rate. (Six animals in each group; the ninth point for groups I and II is the first extinction trial.)

The analysis was made by Fisher's method of fitting curved regression lines (4). For each animal a curved regression line of the form $Y = A + BX + CX^2$ (where Y is the measure of time and X the corresponding trial) was determined for the learning trials, the first eight extinction trials, and the relearning trials. The term, CX^2, provided a measure of the linearity of the data. If the addition of this term to the linear regression line ($Y = A + BX$) did not provide a statistically reliable reduction in the variability of the obtained

[2] The reciprocal was chosen because it tends to minimize the relative discrepancies between latency scores when the latter are large, i.e., when the animals are just beginning to learn and their latencies are most variable. Thus the use of reciprocals makes the dependent variate more likely to be equally and normally distributed at each stage of learning and extinction.

data about the fitted curve, it was assumed that the actual data were represented by the linear function. Of the 24 animals in this study, the addition of the CX^2 term resulted in a reduction of variability significant at the one percent level in no cases during learning and in only two cases during extinction and one during relearning. Consequently, only the linear regression lines were used in the group comparisons, and the variability accounted for by the curvature was combined with the corresponding error term.

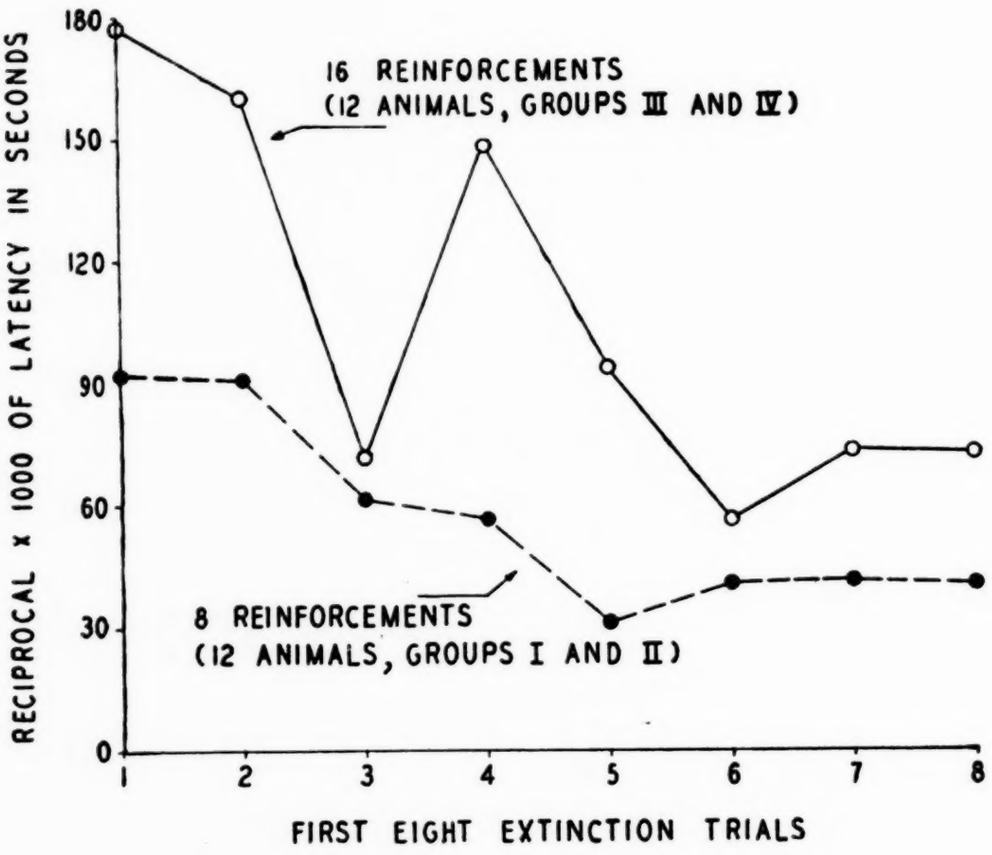

FIG. 2. First eight extinction trials. The animals with 16 reinforcements were more resistant to extinction than those with only 8. An average of 25.0 and 13.2 trials respectively was required to bring these two groups to the 5 min. (i.e., reciprocal × 1000 = 3.3) criterion of extinction.

In the analysis of the first eight extinction trials and the relearning trials (Table IV) all four groups were compared. During learning, however, separate analyses were made of the 16- and 8-reinforcement groups (Table III). This was necessitated by the varying number of trials. In addition, special analyses were made of the first learning trial to demonstrate the initial equality of the four groups, and of the first relearning trial to test for spontaneous recovery. The learning, extinction and relearning curves are shown in Fig. 1, 2, and 3. The relevant means and slopes are summarized in Table II.

The main results of this analysis were:

a. Equality on first trial.—An analysis of variance of the speeds for the four groups on the first learning trial indicated that neither the means of the pellet groups nor the means of reinforcement groups were significantly different. This comparability of the four groups on the first trial is shown graphically in Fig. 1 where the first trial means are included in the learning curve for each group.

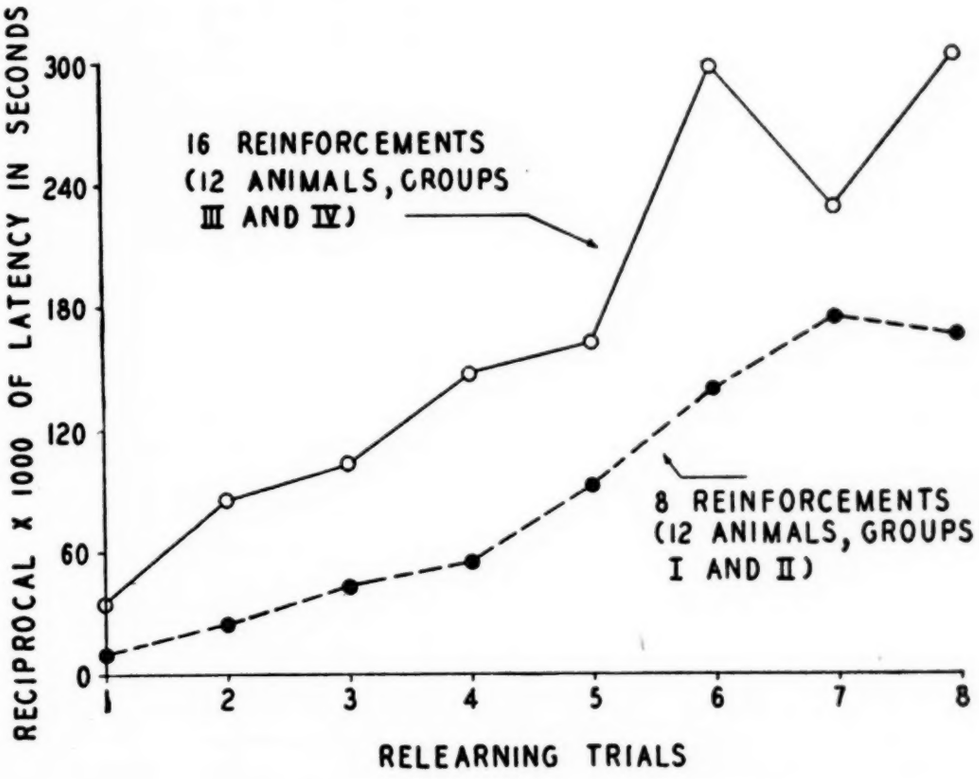

FIG. 3. Relearning trials. The animals that had received more reinforcements showed more spontaneous recovery and relearned better, but the differences were not statistically reliable.

b. Individual differences.—As can be seen from the *F* ratios in Tables III and IV, one of the most consistent findings of the analysis of performance curves was the relatively large differences between animals treated alike. The mean speeds of the animals within a group were significantly different, at the one percent level of confidence, during learning, extinction and relearning. Similarly the slopes of the curves of different animals within the same group are significantly different during the relearning trails although not during extinction. During original learning the curves of the 8-reinforcement animals do not differ significantly in slope but those of the 16-reinforcement animals do. As a result of these large, statistically reliable, individual differences, the group differences must be tested against differences between animals treated alike rather than against the 'discrepance'

TABLE II

MEANS AND REGRESSION COEFFICIENTS EXPRESSED AS 'SPEEDS' FOR VARIOUS PHASES OF LEARNING, EXTINCTION AND RELEARNING

	N	Mean	Regression Coefficient
Learning Trials:			
First Trial			
Group I	6	12.1	
Group II	6	21.4	
Group III	6	10.0	
Group IV	6	9.9	
First 9 Trials			
Group I (1 pellet)	6	52.0	10.6
Group II (4 pellet)	6	57.1	8.5
First 17 Trials			
Group III (1 pellet)	6	104.9	12.2
Group IV (4 pellet)	6	114.1	13.3
Extinction Trials (Trials 2–8):			
Combined 1 pellet groups	12	72.0	9.0
Combined 4 pellet groups	12	76.8	10.9
Combined 8 reinforcement groups	12	51.9	7.3
Combined 16 reinforcement groups	12	96.9	12.6
Relearning Trials:			
Spontaneous Recovery (1st Trial)			
Combined 1 pellet	12	32.9	
Combined 4 pellet	12	10.9	
Combined 8 reinforcement	12	9.3	
Combined 16 reinforcement	12	34.5	
8 Trials			
Combined 1 pellet	12	141.6	36.0
Combined 4 pellet	12	117.4	28.0
Combined 8 reinforcement	12	88.4	25.9
Combined 16 reinforcement	12	170.6	38.1

or error term. This loss in precision and efficiency suggest that more methodological studies should be made of ways to control the heterogeneity of animals within a group.

c. Learning, extinction and relearning curves.—An inspection of Table II and of Figs. 2 and 3 indicates that the differences in mean speed between the 16- and 8-reinforcement groups during extinction and relearning are in the expected direction, i.e., the 16-reinforcement group tends to be faster than the 8-reinforcement group. Both of these differences are statistically unreliable, however, in large part because of the significant variability between animals treated alike (see Tables III and IV). The differences between the group slopes of the 16- and 8-reinforcement groups are also statistically unreliable. When the one- and four-pellet groups are compared during learning, extinction and relearning, their mean and slope differences are even smaller and less reliable.

d. Spontaneous recovery.—Twenty-four hours after the extinction series all animals were given eight additional reinforced trials with one pellet of reward per trial. The animal's speed on the first relearning trial was taken as a measure of spontaneous recovery. The analysis of these speeds is shown in Table V. It is apparent that while the 16-reinforcement group tends to be faster on this trial than the 8-reinforcement group, the difference is not significant at the five percent level of confidence (F_1). The difference between the one- and the four-pellet groups (F_2) is even smaller and less reliable than the differences between reinforcement groups.

TABLE III

ANALYSIS OF VARIANCE OF INDIVIDUAL CURVES DURING LEARNING

Item	Groups I and II 9* Learning Trails (F Ratios)		Groups III and IV 17* Learning Trials (F' Ratios)	
	d.f.	Variance	*d.f.*	Variance
Combined Groups				
1. Slope	1	65,668	1	796,645
2. Variation of Trial Means	7	322	15	10,047
Differences Between Pellet Groups				
3. Means	1	696	1	4,309
4. Slopes	1	804	1	1,740
5. Variation of Trial Means	7	375	15	7,306
Differences Between Animals				
6. Means	10	3,404	10	49,615
7. Slopes	10	857	10	24,187
8. Discrepance	70	873	150	6,396

$F_1 = \frac{\text{Item 6}}{\text{Item 8}} = 3.90^{**}$ $F_2 = \frac{\text{Item 7}}{\text{Item 8}} = .98$ $F'_6 = \frac{\text{Item 6}}{\text{Item 8}} = 7.76^{**}$ $F'_6 = \frac{\text{Item 7}}{\text{Item 8}} = 3.78^{**}$

$F_3 = \frac{\text{Item 3}}{\text{Item 6}} = .20$ $F_4 = \frac{\text{Item 4}}{\text{Item 8}} = .92$ $F'_7 = \frac{\text{Item 3}}{\text{Item 6}} = .09$ $F'_8 = \frac{\text{Item 4}}{\text{Item 7}} = .07$

* The first non-reinforced trial was included as a measure of learning.

** Indicates significant at one percent level of confidence.

A summary of the main results is presented graphically in Figs. 1, 2, and 3. It can be seen that the four groups all learned at approximately the same rate, indicating that the number of pellets had no marked effect, and that the 8- and 16-reinforcement groups were adequately randomized. The 16-reinforcement group showed more resistance to extinction, more spontaneous recovery and better relearning, than did the 8-reinforcement group, but only the first of these three differences was statistically reliable.

TABLE IV

ANALYSIS OF VARIANCE OF INDIVIDUAL CURVES DURING THE EXTINCTION TRIALS* (2 THROUGH 8) AND THE EIGHT RELEARNING TRIALS

Item	Extinction Trials (F Ratios)		Relearning Trials (F' Ratios)	
	d.f.	Variance	*d.f.*	Variance
Combined Groups				
1. Slope	1	67,144	1	1,032,300
2. Variation of Trial Means	5	10,116	6	9,641
Differences Between Pellet Groups				
3. Means	1	963	1	16,718
4. Slopes	1	595	1	16,099
5. Variation of Trial Means	5	4,428	6	24,828
Differences Between Reinforcement Groups				
6. Means	1	84,641	1	324,360
7. Slopes	1	4,685	1	36,987
8. Variation of Trial Means	5	5,698	6	7,745
Interactions***	7	2,951	8	8,374
Differences Between Animals				
9. Means	20	29,773	20	83,995
10. Slopes	20	4,216	20	32,867
11. Discrepance	100	3,132	120	10,430

$F_1 = \frac{\text{Item 9}}{\text{Item 11}} = 9.51^{**}$ $F_2 = \frac{\text{Item 10}}{\text{Item 11}} = 1.35$ $F'_7 = \frac{\text{Item 9}}{\text{Item 11}} = 8.05^{**}$ $F'_8 = \frac{\text{Item 10}}{\text{Item 11}} = 3.15^{**}$

$F_3 = \frac{\text{Item 6}}{\text{Item 9}} = 2.84$ $F_4 = \frac{\text{Item 7}}{\text{Item 11}} = 1.50$ $F'_9 = \frac{\text{Item 6}}{\text{Item 9}} = 3.86$ $F'_{10} = \frac{\text{Item 7}}{\text{Item 10}} = 1.13$

$F_5 = \frac{\text{Item 3}}{\text{Item 9}} = .03$ $F_6 = \frac{\text{Item 4}}{\text{Item 11}} = .19$ $F'_{11} = \frac{\text{Item 3}}{\text{Item 9}} = .20$ $F'_{12} = \frac{\text{Item 4}}{\text{Item 10}} = .49$

* It was necessary in the case of three animals who had reached the five minute criterion before the eighth trial to give them arbitrarily the criterion score of five minutes on the last trial or two.

** Indicates significant at one percent level of confidence.

*** These interactions were originally analyzed into the components due to mean, slopes and variation of trial means. As none of these approached the five percent level of confidence, they were combined for ease in presentation.

TABLE V

ANALYSIS OF SPONTANEOUS RECOVERY SPEEDS (1ST TRIAL OF RELEARNING) FOR GROUPS I, II, III AND IV

	d.f.	Variance
Reinforcement Groups	1	3,813
Pellet Groups	1	2,924
Interaction	1	1,509
Error	20	1,381

$F_1 = \frac{3813}{1381} = 2.76$ $F_2 = \frac{2924}{1381} = 2.12$

Discussion

Finger (**2**) found an inverse relationship between the number of reinforced trials and resistance to extinction. The present study has demonstrated that this paradoxical relationship can be changed to the theoretically expected, positive, one by introducing distinctive cues which differentiate the starting and goal ends of the sequence and thus remove a possible source of conflict counteracting the effects of reinforcement. The procedure of introducing the distinctive cues into the present experiment involved handling the animals between trials. That the positive relationship between reinforcement and resistance to extinction is not dependent on handling per se, however, is indicated by the fact that the previous studies demonstrating such a relationship used bar pressing techniques which did not involve handling the animals between trials (**11, 16, 18, 20**).[3]

Having secured evidence supporting the contention that the paradoxical results in Finger's experiment were produced by a lack of distinctive cues, the next problem is to attempt to see in more detail how the lack of these cues could have produced such results. One possible explanation is as follows. In Finger's experiment the use of the same box alternately as goal and starting box resulted in a sequence of events that was similar to the sequence in an endless maze; an animal is placed in the starting box, the door opens, he runs to the opposite end, searches for food, eats, the door opens, he runs to the opposite end, eats, and so on. Consequently, Finger was actually training his animals in two tendencies that were incompatible when aroused simultaneously; one was to search for food in the goal box and the other was to leave the goal box when the door was opened.[4] During learning these two tendencies were not in conflict. The presence of food in the goal box ensured the act of eating; this in turn supplied a distinctive pattern of cues that (a) terminated searching behavior, and (b) provided a preparatory cue for the opening of the door. But during extinction when the distinctive cue of food was no longer present, the two tendencies were in conflict.[5] The opening of

[3] Nevertheless the authors are sorry that they did not take the extra trouble to introduce the distinctive cues by some method, such as hinging the sides of the goal box so that they could be turned down or folded up, which would have enabled them directly to rule out the variable of handling in the present experiment.

[4] A somewhat similar explanation can be worked out using the acquired reward value of the goal box which would tend to reinforce retracing when it was used as a starting box during extinction.

[5] This might also explain why in other experiments with the same technique (**3, 15**) the decrement for a 16-reinforcement group during a one day interval was so great that their retention of the habit was no better than that of an 8-reinforcement group. The cue of just having eaten in the goal box would be absent, of course, on the first trial testing for retention. Since an extinction series was used in this test, the cue of having eaten was also absent for subsequent trials. The importance of the stimulus trace of eating at the goal could be tested by repeating the experiment using a rewarded instead of an extinction series. The 16-reinforcement group should gain more on the second trial than the 8-reinforcement one.

the door coincided with continued searching behavior and so conflict resulted.

One further assumption is necessary. It must be assumed that the strength of the tendency to leave the starting box reached its maximum early in training whereas the strength of the tendency to remain in the box reached its maximum later in training. This would mean that the 8-reinforcement group would have a relatively strong tendency to leave the starting box and only a weak tendency to remain in the box; thus, the 8-reinforcement group would extinguish relatively slowly. In the 16-reinforcement group the two tendencies would be more nearly equal in strength and therefore this group would extinguish relatively rapidly.

In our experiment where the animal never learned to search for food in the starting box, there would not be any conflicting tendencies.[6] Consequently resistance to extinction would be directly related to the number of reinforcements.

This study of the relationship between reinforcement and resistance to extinction illustrates the fact that, in addition to manipulating physical variables systematically, it is important to attempt to make a theoretical analysis of the factors involved in an experiment. Removing distinctive cues simplifies the physical situation but it does not necessarily produce a purer case better adapted to experimental analysis. An analysis in terms of behavior theory shows that removing these cues can complicate the situation just as the physical simplification of removing all street numbers and signs would complicate the task of learning to find places in a strange city.

The second factor considered in the present study was the relationship between amount of reward and the curves of learning, extinction and relearning. Previous investigations (**1, 9, 19**) have shown that there is a positive relationship between the rate of learning and the amount of reward per trial. Consequently in the present experiment, one would expect that the four-pellet group (.16 gm.) would tend to learn faster than the one-pellet group (.04 gm.). Actually, however, the behavior of animals treated alike was so variable that no reliable differences were found either during learning, extinction or relearning. During learning the mean differences tend to be in the expected direction, but the differences are small as compared to variability of individual animals. During extinction the four-pellet group again shows a shorter mean latency and requires on the average more trials to reach the criterion of extinction than does the one-pellet group, but again the differences are not reliable. During relearning the difference in mean latencies is reversed, but not significantly so.

[6] The starting box would also be expected to have much weaker acquired reward value.

The third factor inadvertently demonstrated by this study is the need for the control of individual differences in the performance of animals within a group. The analysis used in this study is relatively efficient from the statistical viewpoint in that it permits the use of all the data and corresponding degrees of freedom. This increase in efficiency was largely nullified, however, because the significant differences between animals treated alike made it necessary to use estimates of variance based on these differences rather than the error term to test the significance of differences between groups. These estimates based on between individual variation have relatively few degrees of freedom and, therefore, are unreliable estimates; furthermore, they contain sources of variability that presumably could be eliminated by the use of animals from inbred strains, statistical controls such as the analysis of covariance, or other methods of increasing homogeneity. Methodological studies are needed to determine whether any of these techniques can materially decrease the effect of individual differences.

Summary

1. A previous investigator (**2**) had found an inverse relationship between the number of reinforced trials during learning and resistance to extinction. He used the technique of having hungry animals run from a starting box down a short, narrow elevated runway to a goal-box which was identical with the starting-box. In order to minimize handling the animals, the two boxes (with the animal in the one that had been at the goal) were exchanged between trials. The main purpose of the present experiment was to determine whether or not the paradoxical results of that study were produced by a lack of distinctive cues differentiating the starting box from the goal. A subsidiary purpose was to determine the relationship between amount of reward and resistance to extinction.

2. The main features of apparatus and procedure were the same as those used by the previous investigator except that cues of various kinds were introduced to differentiate clearly the starting-box from a goal platform.

3. Twelve hungry animals were each given eight successive reinforced training trials; 12 others were given 16 such trials. Half of each group received one pellet of food (.04 gm.) as a reinforcement at the end of each run; the other half received four such pellets. Immediately after this training all animals were given non-rewarded trials. The time spent in the starting-box and on the first eight in. of the runway was recorded as a latency score for each trial. Extinction trials were continued to a criterion of two successive scores

of five min. The next day all animals were given eight rewarded trials to test for spontaneous recovery and relearning.

4. An analysis of variance performed on the data showed that the introduction of the distinctive cues (and thus, the elimination of a source of confusion) had changed the results from an inverse to a direct relationship between number of reinforced trials and resistance to extinction. The average number of non-rewarded trials required to reach the five min. criterion was 25.0 for the animals with 16 reinforcements and 13.2 for those with eight; for a three min. criterion the averages were 19.7 and 10.5 respectively. At both of these criteria the differences were beyond the one percent level of significance. Furthermore, the animals with more reinforcements showed shorter latencies during the first part of extinction, more spontaneous recovery on the first trial a day later, and better relearning scores than those with fewer reinforcements, but these differences were not statistically reliable.

5. Although the animals receiving four pellets on each rewarded trial were superior to those receiving one pellet in their latency scores during learning and extinction and in the number of trials run before reaching each criterion of extinction, none of the differences between the two groups was statistically reliable.

6. Analyses of the curves of learning, extinction and relearning showed statistically significant differences among animals treated alike. Since these relatively large individual differences greatly reduce the efficiency of an experiment, methodological studies of ways of controlling such differences would be valuable.

(Manuscript received February 17, 1947)

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SOME FUNCTIONAL RELATIONSHIPS OF REACTION POTENTIAL ($_sE_R$) AND RELATED PHENOMENA[1]

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INTRODUCTION

Recently a methodology has been developed for quantifying reaction potential (1). This at once opens the way to a corresponding empirical investigation of the functional relationships of habit strength (primary motivation constant) to several other fundamental behavioral processes. One such relationship of great systematic importance is that of reaction potential in simple, single-link learning to the number of reinforcements, since this presumably constitutes the basic or primary curve of learning from which all other curves of learning are in one way or another derived.[2] Another basic relationship of reaction potential is that to reaction latency ($_st_R$), which would conceivably be very useful as an economical means of determining reaction potential since latency is far easier to determine than is $_sE_R$. And dependent upon the above relationships there are the determinations of: (1) the distance of the absolute zero of reaction potential (Z) below the reaction threshold, and (2) the maximum reaction potential (M') attainable under ordinary conditions. Incidentally these analyses of the reaction-potential results will presumably yield several bits of factual evidence bearing on the fertility and general validity of the method of quantification.

The present article is based on the same experimental work as were the two previous ones in this series (1, 8). It may here be said merely that 59 male albino rats were trained at 22 hours' food priva-

[1] This is the third of a series of four related studies (1, 8) by the present authors. The experimental work was performed by Mr. Felsinger; the statistical work and curve-fitting were carried out by Mr. Gladstone and Mr. Yamaguchi; Mr. Hull supervised the investigation generally, prepared the manuscript, and takes responsibility for any errors of interpretation.

[2] Relatively simple examples of the complexities which learning generates are seen in compound trial-and-error learning. The reactively heterogeneous form (6, 7) involves clearly marked competitional interferences of the primarily learned reaction potentials through stimulus generalizations; and the homogeneous form (10) involves summational effects. It is obvious that where many reaction potentials are being formed at the same time and are interfering or summating with each other, it is not feasible to isolate any one of them for systematic quantification in such a way as to determine the quantitative law of its formation. In a somewhat analogous manner the curve of simple trial-and-error learning is complicated by the experimental extinction of the erroneous tendencies in the situation, the primary law of which has not yet been determined.

tion with one trial per day to move a manipulandum $\frac{1}{16}$ in. to the left in order to receive a small cylinder of food. The main data utilized in the present report are the reaction latencies of the animals at the successive daily trials.

The results with which the present study will be mainly concerned are (1) the reaction potentials derived from the raw latency data (unequalized for the different rates of learning displayed by the individual animals) at 25 selected stages of the learning process, and (2) the median latencies at the same 25 stages of learning as calculated from the same (raw) data from which the reaction potentials were calculated. These results are presented in Table I.

TABLE I

ORDINAL NUMBER ($\dot{N}$) OF SUPERTHRESHOLD REINFORCEMENTS, THE PARALLEL REACTION POTENTIALS AS CALCULATED BY A SPECIAL METHODOLOGY DESCRIBED IN DETAIL ELSEWHERE (8), AND THE CORRESPONDING MEDIAN RAW REACTION LATENCIES

Ordinal Number ($\dot{N}$) of Selected Superthreshold Reinforcements*	Reaction Potentials ($_{S}E_{R}$) of the 59 Organisms as a Group	Medians of the Raw Reaction Latencies ($_{S}t_{R}$)
0	.000	25.00
2	.323	8.31
4	.810	4.45
6	1.148	3.35
8	1.549	2.01
10	1.850	1.36
12	1.689	1.89
14	1.924	1.15
16	2.191	.95
18	2.380	.90
20	2.398	.90
22	2.205	.96
24	2.520	.80
26	2.744	.70
28	2.418	.90
30	2.574	.70
33	2.969	.65
36	2.621	.70
39	2.638	.72
42	3.171	.61
45	2.796	.62
48	3.139	.60
51	3.153	.65
54	3.201	.57
57	3.279	.54

* It is important to note that the number of the superthreshold reinforcements ($\dot{N}$) is one less than the number of the superthreshold trials (N) employed in the two previous studies by the present authors, and *two* less than the median number of reinforcements from the very beginning of learning ($\dot{N}$). This means that

$$\dot{N} = N + 2 = N + 1.$$

Reaction Potential as a Function of the Number of Superthreshold Reinforcements

We begin our investigation of the functional relationships of reaction potential by determining its relation to the number of superthreshold reinforcements (N), i.e., those reinforcements which take place after the occurrence of the first reaction evoked by the conditioned stimuli alone. In the present situation the conditioned stimuli are those arising from the apparatus, i.e., the experimental chamber, the raising of the shutter, the manipulandum thus exposed, together with the stimulus traces produced by taking the animal from the living cage and placing it in the apparatus.

The essential data available for the determination of this function are presented in columns 1 and 2 of Table I. The functional relationship is represented graphically by the positions of the series of 25 solid circles ($_sE_R$'s) shown in Fig. 1. The first approximation to

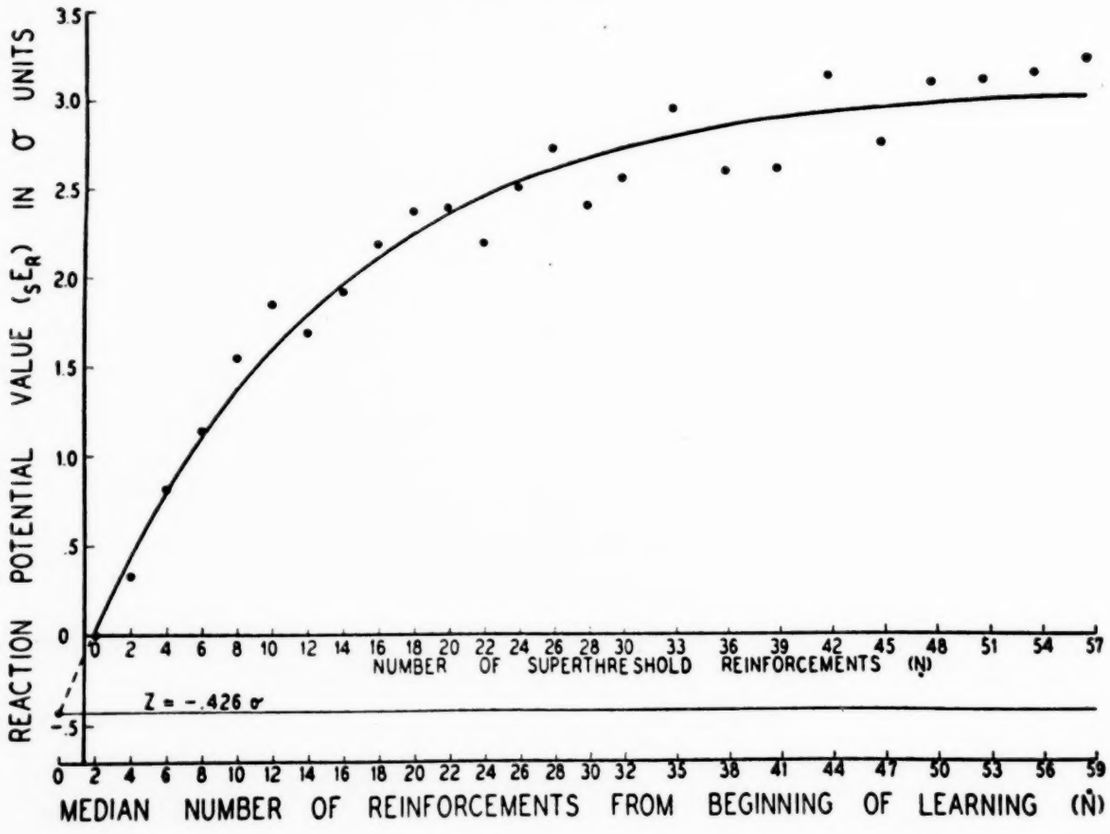
REACTION POTENTIAL VALUE ($_sE_R$) IN σ UNITS
3.5
3.0
2.5
2.0
1.5
1.0
.5
0
-.5
0 2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 33 36 39 42 45 48 51 54 57
NUMBER OF SUPERTHRESHOLD REINFORCEMENTS (N)
Z = -.426 σ
0 2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 32 35 38 41 44 47 50 53 56 59
MEDIAN NUMBER OF REINFORCEMENTS FROM BEGINNING OF LEARNING (Ñ)

Fig. 1. Graphic representation of the functional relationship of reaction potential ($_sE_R$) to the number of superthreshold reinforcements (N) preceding each. The smooth curve running among the circles was plotted from the $_sE_R$ values estimated from the N's by means of the fitted equation. The broken-line extension of the curve at the lower left represents the extrapolation of the learning curve into the subthreshold region to the absolute zero of reaction potential (Z).

the presumptive behavioral law implicit in these two columns of data, and likewise in the series of circles of Fig. 1, is explicitly stated by

the equation which best expresses the quantitative relationship of the number of superthreshold reinforcements (N) to the corresponding reaction potentials. Three different types of equation gave promise of satisfying this condition reasonably well. The one finally chosen as probably the best first approximation to the true law (**5**, p. 8) is:

$$_sE_R = 3.084(1 - 10^{-.0305N}) + .04. \qquad (1)$$

Substituting the N values of Table I successively in this equation we secure a series of estimates of $_sE_R$ corresponding to each of the 25 $_sE_R$ values in Table I. The smooth curve running among the circles of Fig. 1 is plotted from these estimated values. The approximation of the curve to the circles indicates the goodness of the fit. It is obvious that no simple function could be found to conform to the extremely irregular series of values, especially from $N = 22$ to $N = 48$; this irregularity is presumably due in the main to the limitation in the size of the sample of data (59 Ss). Nevertheless the curve seems to be a fairly good fit except for the last three or four points beyond $N = 50$ or so. Numerous other combinations of constants were tried out systematically with this equational form and tested by the method of the least square deviations of the estimates from the empirical values, and equation (1) was selected as the best; its mean square deviation was .0275 σ^2.

The Absolute Zero (Z) of Reaction Potential

It is quite clear that the value arbitrarily taken as zero in our determination of reaction potential (**8, 2**, p. 228) is not the zero in any absolute sense, any more than the zero of the centigrade thermometer is the absolute zero of temperature. This is shown by the fact that more than one reinforcement is usually required to bring the reaction potential above the response-evocation threshold (**5**, p. 6). Nevertheless, quantitative behavior systematization demands a location of the distance from the practically adopted zero to the true or absolute zero[3] of reaction potential (Z), very much as a satisfactory development of the science of thermodynamics required the location of the absolute zero in temperature. Moreover, an additional parallelism between our present problem and the physics of temperature lies in the fact that the method which we shall utilize in locating Z involves (1) the use of functions established empirically in the region where observations are practicable, and (2) the extrapolation of them to the zero point which cannot be directly determined.

[3] The term 'absolute' as applied to this zero does not imply anything as to the accuracy of its determination. It is intended rather to emphasize by contrast the difference of the status of this zero from that of the clearly arbitrary zero of the ordinate scale of Fig. 1.

Our method of locating Z[4] is closely related to the most convincing observations bearing on its existence. In the original investigation which yielded the reaction latency data from which the reaction-potential values presented in Table I were derived, the number of reinforcements required by each of the 59 animals was recorded. Both the mean and the median of the subthreshold reinforcements for the group of Ss as a whole was found to be slightly less than 2. Since no great exactness can be attributed to this determination in the present experiment, the integral value of 2 will be utilized in our calculations. This will simply be substituted in the equation (1) just fitted to the relationship ${}_sE_R = f(\dot{N})$.

In this connection let us explicitly clarify a matter of symbolism which otherwise may cause confusion. Our first study (1) used the symbol N to indicate the ordinal number of conditioned reaction evocations, i.e., the number of superthreshold *trials*. But the number of superthreshold *reinforcements* at the occurrence of the first conditioned response is clearly one less than the number of superthreshold trials because in the present type of learning situation reinforcement follows the response reinforced; the first indication that the threshold has been passed is the occurrence of the conditioned reaction, and at this time no superthreshold reinforcement has taken place at all. Thus $\dot{N} = N - 1$. However, the fact that a median of two reinforcements occurred previous to the first reaction evocation means that

$$\dot{N} = \dot{N} + 2. \tag{2}$$

Letting $\dot{N}$ equal zero, and substituting in equation (2), we have

$$0 = \dot{N} + 2,$$

i.e.,

$$\dot{N} = -2.$$

Now, substituting this value of $\dot{N}$ in equation (1), on the assumption that subthreshold reinforcements are comparable with those above the threshold, we have

$$\begin{aligned} {}_sE_R &= 3.084(1 - 10^{-.0305(-2)}) + .04 \\ &= 3.084(1 - 10^{.061}) + .04. \\ \therefore \quad {}_sE_R &= -.426. \end{aligned}$$

But, on the present assumptions, previous to any reinforcements whatever the associated reaction potential must equal absolute zero or Z, i.e., when $\dot{N} = 0$, it follows that ${}_sE_R = 0$. We therefore conclude that

$$Z = -.426. \tag{3}$$

[4] The second method of determining Z is described below.

This extension of the curve of learning into the subthreshold region is represented by a broken line in Fig. 1, and the circle marking the point is empty to symbolize the indirect nature of the determination.

The Learning Equation Based on the Absolute Zero of Reaction Potential and the Values of M' and ${}_sL_R$

It is now necessary to transform equation (1) into an equivalent equation based upon the absolute zero of reaction potential. An inspection of equation (1) shows that when $N = 0$, ${}_sE_R = +.04$. It follows that Z falls $.426 + .04$, or $.466\ \sigma$ below the *fitted* value of the first superthreshold reaction. Therefore the coefficient of the new equation must be $3.084 + .466$, or 3.550. Accordingly the new equation is

$$ {}_sE_R = 3.55(1 - 10^{-.0305\dot{N}}). \tag{4} $$

This means, for one thing, that the scale values at the left of Fig. 1 must all be increased by $.426\ \sigma$ to conform to equation (4). It also means (**5**, p. 9) that the maximum value of reaction potential (**4**, pp. 134, 405), theoretically at least, is 3.55, i.e.,

$$ M' = 3.55\ \sigma. \tag{5} $$

With equation (4) available it becomes possible to calculate the true reaction threshold (${}_sL_R$), at least in theory (**5**, p. 9). It may have been noted above that the reaction at trial $N = 1$ was called the first superthreshold reaction but not that it marked the reaction threshold. This distinction will be clear from the following considerations. The learning process occurs by finite increments or steps ($\Delta {}_sE_R$). Since different organisms presumably have different reaction thresholds as well as different rates of learning, it will happen with an occasional organism that the last increment of reaction potential before the reaction threshold is passed will leave no reaction potential to spare. On the other hand, another organism at the first superthreshold reaction may have almost an entire increment of reaction potential in excess of that required to pass the reaction threshold. The reaction potentials of the remaining subjects will naturally be scattered midway between these two extremes. The median organism passing the reaction threshold (${}_sL_R$) would thus fall halfway between, with an excess of half an increment of reaction potential. Now, the point midway between just barely passing the reaction threshold and not quite passing it is equivalent approximately to the influence of half of a reinforcement. This means that upon the whole the reaction threshold falls at the equivalent of half a reinforcement below the actual median number of re-

inforcements required to cross the threshold. But two subthreshold reinforcements less .5 of a reinforcement yields a value of

$$\dot{N} = 2 - .5 = 1.5.$$

Substituting this value of $\dot{N}$ in equation (4), we have

$$\begin{aligned} {}_sL_R &= 3.55(1 - 10^{-.0305 \times 1.5}) \\ &= 3.55 \times .1. \\ \therefore \quad {}_sL_R &= .355\,\sigma. \end{aligned} \tag{6}$$

As a final determination in this connection we may wish to express M' in terms of the reaction threshold. This can easily be done since both M' and ${}_sL_R$ are expressed in the same scale unit. Accordingly,

$$\begin{aligned} \frac{{}_sL_R}{M'} &= \frac{.355}{3.55} = .100. \\ \therefore \quad {}_sL_R &= .10\,M'. \end{aligned} \tag{7}$$

It thus happens that according to the present tentative data, ${}_sL_R$ stands at about what had previously been conjectured (4, p. 327).

The Formal Quantification of the Hab

A certain amount of skepticism has been expressed as to the possibility of formally quantifying the hab (4, p. 114). It happens that with equation (4) at our disposal some concrete suggestions not previously possible may now be made regarding the quantitative mathematical treatment of this concept. These suggestions must be very tentative, however, because they depend upon an important assumption which as yet lacks proof. This is that reaction potential is a multiplicative function jointly of habit strength and drive, i.e., that (4, p. 242)

$${}_sE_R = D \times {}_sH_R.$$

On this assumption together with the assumption that the form, at least, of equation (4) will subsequently be verified for simple, uncomplicated learning, we make the additional assumption that the coefficient 3.55 of equation (4) represents the D in the present situation, and that the parenthetical part represents habit strength, i.e., that

$${}_sH_R = 1 - 10^{-.0305\dot{N}}. \tag{8}$$

Taken at face value, this parenthesis without any manipulation whatever presents the natural basis for a centigrade system of habs or habit strength units. For example, by the substitution of the following $\dot{N}$ values in equation (8), the centigrade range of ${}_sH_R$ values

listed below is obtained:

$$\begin{aligned}
\text{when } \dot{N} &= 0, {}_sH_R = .000,\\
\text{when } \dot{N} &= 2, {}_sH_R = .1312,\\
\text{when } \dot{N} &= 4, {}_sH_R = .2453,\\
\text{when } \dot{N} &= 20, {}_sH_R = .7546,\\
\text{when } \dot{N} &= 41, {}_sH_R = .9438,\\
\text{when } \dot{N} &= \infty, {}_sH_R = 1.000.
\end{aligned}$$

If desired the decimal point may be moved two places to the right for certain purposes, e.g., it may be convenient to speak of 13 habs, 25 habs, 75 habs, 94 habs, and 100 habs. It must be emphasized once more that even the present statement is largely programmatic, pending the quantification of a series of reaction potentials in which habit strength is held constant and only the drive is varied (**5**, p. 19 ff.).

Reaction Potential as a Function of Reaction Latency

The general shape of the relationship of ${}_sE_R$ when plotted as a function of ${}_st_R$ is presented by the graphic arrangement of the circles of Fig. 2, which shows a characteristic curvature. As a first ap-

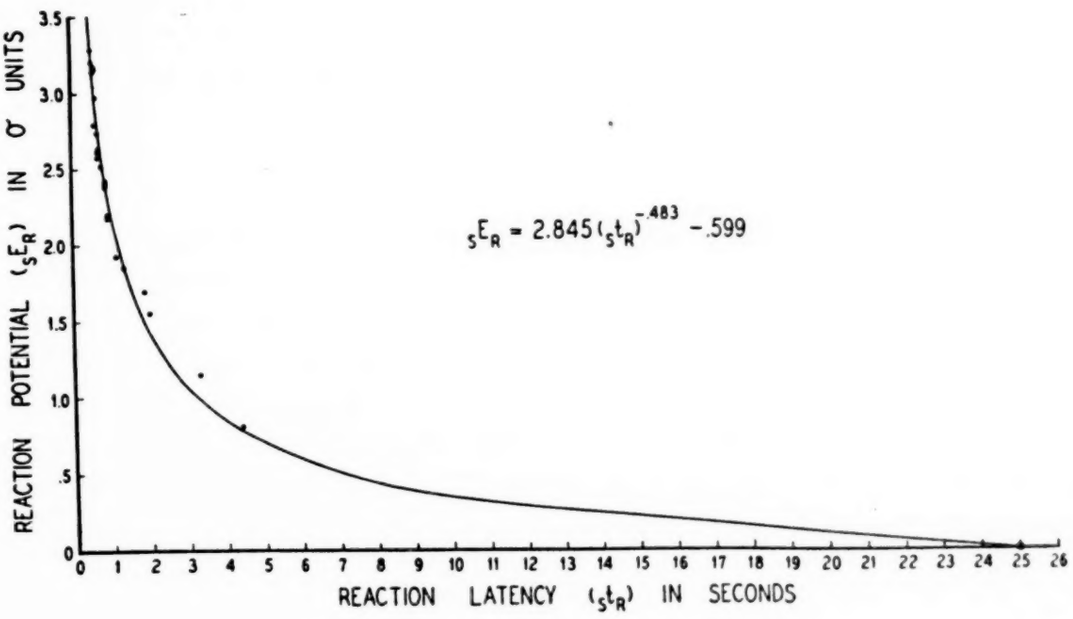

Fig. 2. Graphic representation of the relationship of reaction potential as a function of reaction latency. The dots represent the values from the corresponding columns of Table I. The smooth line running among the empirical data points was plotted from the fitted equation shown just above it.

proximation to a mathematical expression of this empirical relationship, an equation (**5**, p. 13) was fitted to the data thus represented.

Of two types tried, the better fitting equation was

$$ {}_sE_R = 2.845\ {}_st_R{}^{-.483} - .599. \tag{9} $$

The estimated values of ${}_sE_R$ secured by substituting in this equation the empirical latencies of column 2, Table I, have been plotted as a smooth curve running among the dots of Fig. 2. There it may be seen that the fit is fair. The mean of the squared ${}_sE_R$ deviation amounts to .0143 σ^2. This is the minimum secured from systematically varying appropriate combinations of parameters.

For certain purposes it is desirable to know the relationship of ${}_st_R$ as a function of ${}_sE_R$. This may be secured by merely transforming equation (9). The result of this procedure is[5]

$$ {}_st_R = \frac{8.71}{({}_sE_R + .599)^{2.07}}. \tag{10} $$

Reaction Latency as a Function of the Number of Reinforcements from the Beginning of Learning ($\dot{N}$)

We may now raise the question of the functional relationship of ${}_st_R$ to $\dot{N}$ (**5**, p. 11). It will be recalled that in a previous article (**1**) the fitted relationship of the median 'raw' ${}_st_R$'s to N was given as being rather unsatisfactory in the middle region. The data for the determination of this functional relationship are available in Table I, provided 2 be added to each of the N values to convert them into $\dot{N}$ values. These data are represented graphically in Fig. 3. To these values has been fitted the equation

$$ {}_st_R = 74.05\ \dot{N}^{-1.60} + .43. \tag{11} $$

The ${}_st_R$ estimates derived from the $\dot{N}$ values by means of equation (11) are represented by the smooth curve running among the data points in Fig. 3. An inspection of the proximity of this curve to the several points shows that the fit is fairly satisfactory. The mean square deviation of the estimated from the empirical values is only .0265 sec.^{2} Apparently the shift from N to $\dot{N}$ has made a marked difference in the constants required in the equation in order to fit the function, since the equation based on N (**1**) was found to be

$$ {}_st_R = 25.6\ N^{-1.163} + .3. $$

Nevertheless the fit based on $\dot{N}$ is definitely better than the one based on N since the latter equation, for exactly the same ${}_st_R$ values as presented in Table I above, shows a mean square deviation of .0998 sec.^{2}

[5] Equation (10) may be compared with the largely conjectured relationship previously published (**4**, p. 346). The form of the equation is essentially the same but the constants, especially the exponent, are different.

Reaction Potential as a Function of Primary Hunger Motivation (h)

As an additional demonstration of the possibilities opened up by the quantification of ${}_sE_R$ we may present an interpretation, which it makes possible, of certain primary motivation data published some years ago by Perin (9). In the study in question Perin gives the median latency values of four groups of albino rats, all of which had been given 16 reinforcements and tested at 1, 3, 16, and 23 hours of food privation respectively. One of

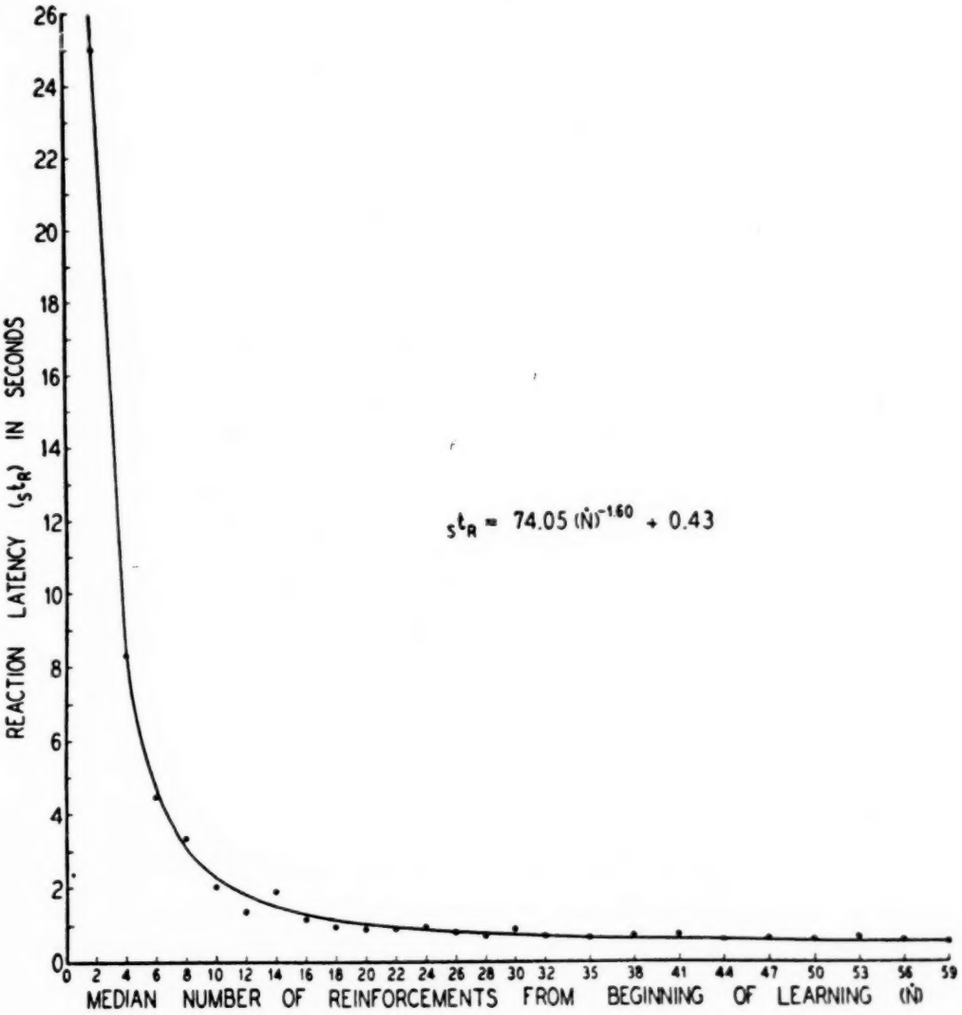

Fig. 3. Graphic representation of the functional relationship of reaction latency (${}_st_R$) to the median number of reinforcements ($\dot{N}$) from the beginning of learning.

these latencies, that at 16 reinforcements and 23 hours' hunger, coincides with the same values in Table I above. Perin's ${}_st_R$ value at that point was .25 sec., and the corresponding value of our data is .95 sec. There is thus a difference of .95–.25, or .7 sec., between these two latencies, doubtless due to such factors as the difference between the respective manipulanda and the method of testing. To secure a rough approximation to an equivalence of the two sets of latency data we therefore add .7 to each of Perin's four latencies.

Substituting these corrected latency values in equation (9), we secure a series of four estimated reaction-potential values which correspond to four periods of food privation (h). These are represented by the circles of Fig. 4.

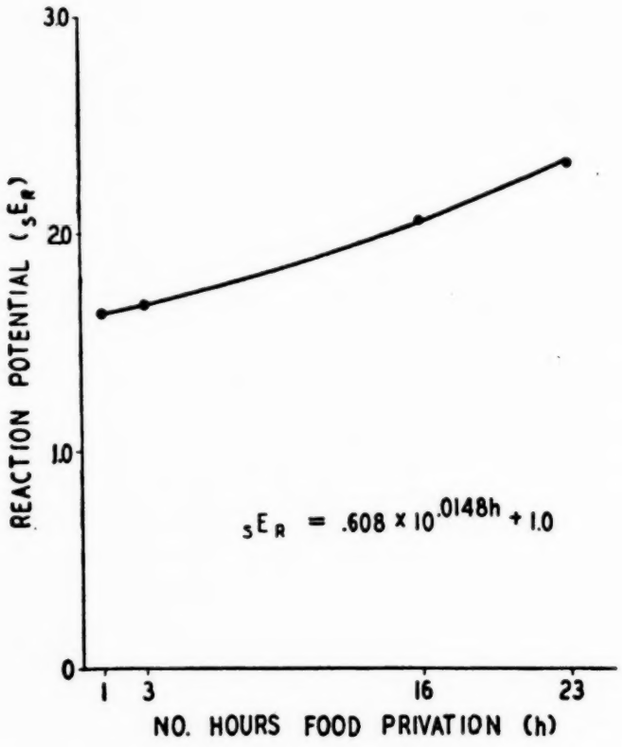

FIG. 4. Graphic representation of what purports to be a first approximation to the molar law of primary motivation. Derived indirectly from latency data published by Perin (9).

An equation found to fit these data closely is

$$_sE_R = .608 \times 10^{.0148\ h} + 1.0, \qquad (12)$$

which purports to be the molar law of primary motivation as a function of the number of hours of food privation over the range covered by Perin's data. The mean square deviation of this fit is .0005. Moreover, with equations (8) and (12) available a second motivation equation, $D = f(h)$, could easily be derived. However, there are too many differences between the Perin experiment and the present one, together with too few points on the curve, for such equations to be regarded as more than first approximations to the laws in question. Nevertheless, these results do serve to illustrate further the general scientific fertility of quantifying $_sE_R$.

DISCUSSION

It was pointed out above that three different equational forms were originally considered as possibilities for the representation of $_sE_R = f(\dot{N})$. The one finally chosen has already been presented. The other two, with the several constants in each having been systematically varied so as to

yield the best fit, are as follows:

$$_sE_R = 5.25\ (\dot{N})^{.135} - 5.75, \tag{13}$$

$$_sE_R = 2.319 \log \dot{N} - .847. \tag{14}$$

The graphic representation of the two functions in relation to the 25 data points involved is shown in Fig. 5. An inspection of this figure reveals that

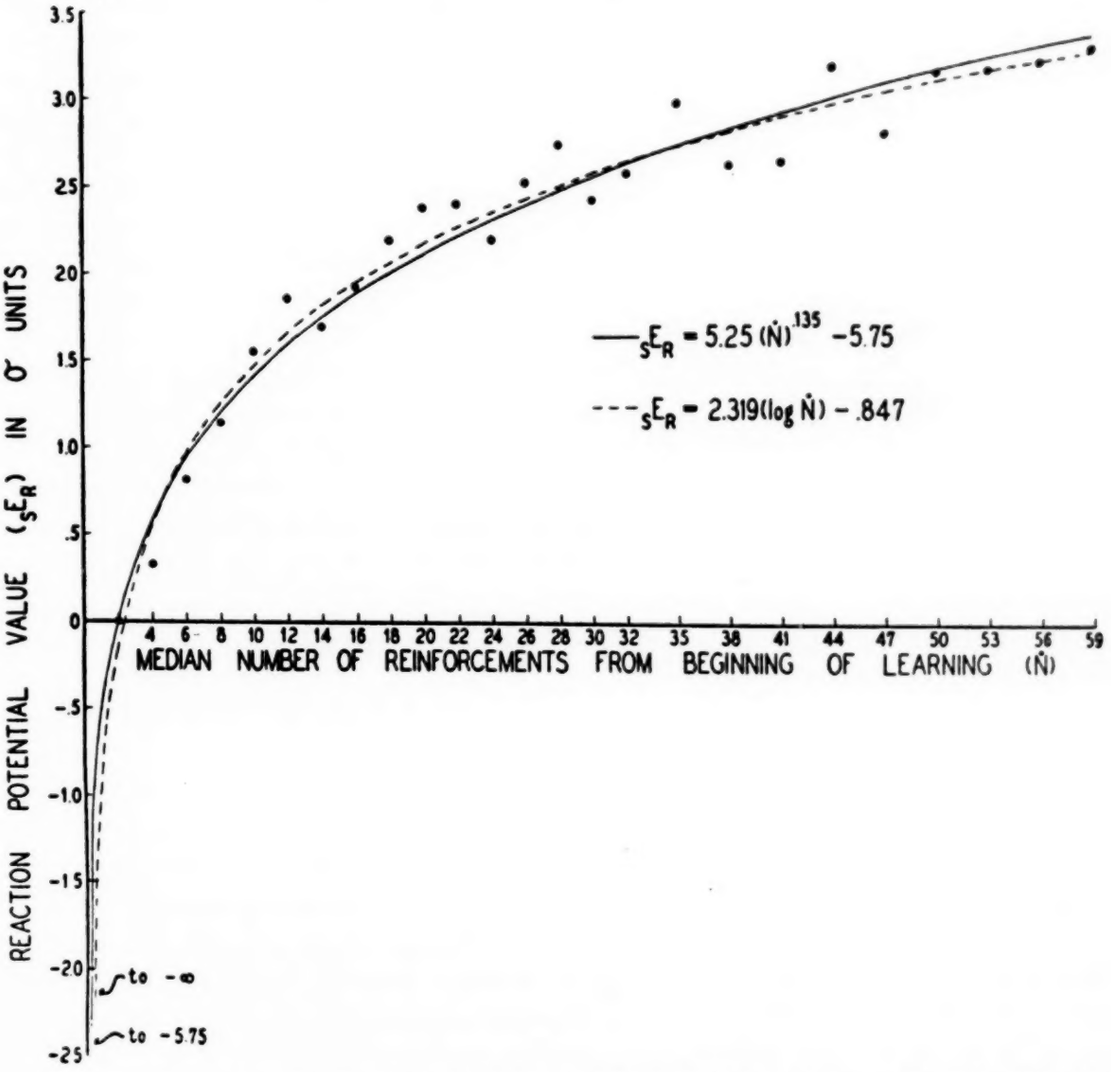
REACTION POTENTIAL VALUE ($_sE_R$) IN σ UNITS
MEDIAN NUMBER OF REINFORCEMENTS FROM BEGINNING OF LEARNING (Ṅ)
$_sE_R = 5.25\ (\dot{N})^{.135} - 5.75$
$_sE_R = 2.319(\log \dot{N}) - .847$
to -∞
to -5.75

FIG. 5. Graphic representation of the fits to $_sE_R = f(\dot{N})$ by the power function (equation 13) and the log function (equation 14). Note particularly the magnitudes of the extensions of the respective functions into the subthreshold region.

both functions not only fit fairly well, but are remarkably similar. This at once raises the question as to why the one was selected and these two rejected. The answer is that no absolute selection has been made or can be made with certainty in the present stage of the evidence. Because of this the relevant facts in the matter will be set down here. The choice involved three considerations or criteria.

The first and most obvious consideration, though not necessarily the most weighty, is the relative excellence or closeness of the fit yielded by the

three equational forms. The mean square deviation of the first or power function, equation (13), is .0293; that of the second or log function, equation (14), is .0246; whereas that of the growth or exponential function finally selected (Fig. 1) is .0275. On this criterion alone the log function clearly should be chosen, the growth function attaining only second place.

The second criterion is that the projection of the reaction potential below the first conditioned-reaction evocation to the absolute zero of reaction potential (Z) must be reasonable, or at least possible. It happens that when $\dot{N} = 0$ is substituted in equation (14) the value of ${}_{S}E_{R}$ becomes $-\infty$. According to our present perspective, at least, such a value would seem to be quite impossible. For this reason the equational form yielding the best fit to the empirical reaction-potential data values must be rejected. The same test when applied to equation (13) yields a Z value of -5.75, which while possible so far as we now know, is surprisingly large since according to this interpretation the learning curve would extend considerably farther below the practical reaction threshold (preliminary zero value) than it does above.

The third criterion is that the simple learning process should approach a genuine maximal asymptote as it is continued. While this is apparently in general harmony with most opinion in the field, as well as with available evidence, no one seems ever to have committed himself specifically on the subject. On the other hand, the criterion does have the support of the concept of physiological limit which is not known to permit infinite functions in any other fields.[6] At all events it is clear that both the power and the log functions fail this third or asymptotic criterion, whatever its significance.

We conclude, then, that while the growth function of ${}_{S}E_{R} = f(\dot{N})$, i.e., equation (4), ranks only second on the first of the three types of equation considered, it satisfies the other two criteria which if sound are both absolute. On this type of reasoning the log function is rejected on two absolute counts and the power function on one absolute count and one comparative count.

Intimately connected with the problem of the precise law of simple learning or habit formation is the question of the soundness of the data upon which the quantifications were based. There is in fact an extremely obvious defect in the learning data from which the quantifications of ${}_{S}E_{R}$ were made. The defect is found in the fact that latency records of animals of quite different rates of learning are pooled without distinction to secure the latency values from which the present ${}_{S}E_{R}$ values were calculated. This difficulty was seen long in advance (5, p. 5). It was planned to avoid it by reducing the latency records of all subjects to comparable-length series by the equally weighted Vincent methodology (3, p. 245) before pooling for quantification purposes, and this was actually done. However, we found that on the application of Thurstone's linearity-of-slope test for the normality of the dispersions involved in the several columns of reaction-potential differences, the Vincentized data did not show up nearly so well as did the ordinary raw

[6] Even so, it does not follow that the asymptote of ${}_{S}E_{R}$ by the reaction-latency criterion here implicitly adopted is necessarily the asymptote by the criterion of salivary reaction or the galvanic skin reaction. It might also be that continued training beyond the latency asymptote would materially increase the resistance to experimental extinction. However, adequately to interpret such an event we shall need to know much more about experimental extinction than we now do.

data, the results of which are reported above. The Vincentized results were accordingly rejected.[7]

Fortunately, there is an obvious and quite effective remedy for the difficulty caused by the different rates of learning of the *S*s utilized in most learning experiments. This is for the investigator to run enough *S*s so that he could reject all but the most numerous group of equally fast learners and still have a sufficient number upon which to make the quantification; probably something like 200 *S*s would be needed for really satisfactory results. In the present experiments, a maximum of 12 *S*s out of 59, or approximately 20 per cent, possessed a common rate of learning by our criterion. According to this it would be necessary to run 1000 *S*s in such an investigation to secure an adequate number possessing approximately the same rate of learning. However, other groups of equally fast learners in the present experiment yielded 9, 10, and 11 *S*s respectively. If 600 *S*s were trained it would be possible at this rate to secure four groups, each group about as large as the one used here and each yielding an undistorted learning curve so far as this problem is concerned. These four curves could then safely be subjected to the equally weighted Vincent process and thus would yield a presumably sound curve of simple learning.

Meanwhile we and others have been assuming that great distortions occur in the learning curves of any given variety, from the pooling of records of *S*s varying normally with respect to rate of learning. So far as we have been able to discover, no one has attempted to substantiate this assumption. In order to secure some indication of the magnitude of this distortion, the actual distribution of fast and slow learners of the present experiment was assumed, with each *S* possessing a perfect simple growth learning curve, to be represented by the equation

$$_{S}E_{R} = 3.2\ (1 - 10^{-.0293\ (N)}).$$

Of the present 59 *S*s, 11 reached the approximate asymptote in 30 trials 6, in 40, 10 in 50, 12 in 60, 9 in 70, 7 in 80, and 4 in 90 or more. Accordingly, $_{S}E_{R}$ values were calculated for the seven speed-of-learning groups at each of seven stages of the learning process on a consistent scale such that each group would reach at the number of trials listed in the preceding sentence a reaction potential exactly equal to that attained by the median group at $N = 60$, i.e., 3.14. The weighted means of the calculated and pooled reaction

[7] It is believed that the reason for the apparent defectiveness of the Vincentized latency data for the present quantificational methodology is that by the method employed the individual scores were usually partitioned and then combined in various proportions to make up the Vincent values (3, p. 245). This process in effect tended partially to average adjacent values, which naturally reduced and so distorted the variability, i.e., the unit of measurement. As a matter of record, the three equations fitted to the Vincent data were

$$_{S}E_{R} = 4.2\ (1 - 10^{-.025\ \dot{N}}) - .32,$$
$$_{S}E_{R} = 4.575\ \dot{N}^{.165} - 5.006,$$
$$_{S}E_{R} = 2.833 \log \dot{N} - 1.126.$$

The mean square deviation of the growth function was .0484, of the power function, .0304, and of the log function, .0313. Thus the growth function proved the least acceptable of the three but all are definitely inferior in fit to the least acceptable of the three equational forms secured from simply pooling the learning data.

potentials at the seven selected stages of learning are presented as the second row of values shown in Table II. This reveals a surprisingly slight dis-

TABLE II

THE PRESUMPTIVE EFFECT OF POOLING THE DATA OF FAST AND SLOW LEARNERS IN DISTORTING THE SIMPLE LEARNING CURVE OF THE GROUP OF *Ss* LEARNING AT THE MEDIAN RATE

Number of Superthreshold Trials of Median Group	0	10	20	30	40	50	60
Reaction potentials of median group	.00	1.57	2.37	2.78	2.98	3.09	3.14
Mean reaction potentials of all groups when pooled	.00	1.71	2.46	2.82	2.99	3.09	3.14
Increase in ${}_sE_R$ over median group due to pooling	.00	.14	.09	.04	.01	.00	.00

tortion from the standard represented by the equation as applied to the median group. The distortion apparently has its maximum in the neighborhood of the tenth trial, tapering off steadily thereafter to a value so small that it does not appear within the first two decimal places. Thus, the pooling of the records of *Ss* with different learning rates produces a genuine distortion somewhere early in the process, but it is believed to be so slight that for most purposes the advantage in precision to be gained from its experimental avoidance will hardly repay a very great amount of labor directed to that end. It may be added that parallel computations based on a normal probability distribution of subjects' training rates yielded considerably less distortion than that shown in Table II.

GENERAL SUMMARY AND CONCLUSIONS

A simple one-link type of learning was carried out with widely distributed trials on 59 albino rats. The latencies of these reactions were recorded to considerably beyond the latency asymptotes of the several *Ss*. These latencies, taken as inverse monotonic indicators of reaction potential, i.e., habit strength varied with primary motivation constant, were utilized in a quantificational methodology. The relationships of the results of this procedure with associated results of the investigation follow.

1. The increase in habit strength, with hunger motivation constant, was found to be fairly well fitted by the equations

$$ {}_sE_R = 3.084(1 - 10^{-.0305N}) + .04 $$

and

$$ {}_sE_R = 3.55(1 - 10^{-.0305\dot{N}}). $$

The second of these equations is tentatively considered as the basic or primitive law of learning, since it purports to be based at the absolute zero of reaction potential.

2. The relationship of habit strength, with hunger motivation constant, to reaction latency in seconds was found to be fairly well fitted by the equation

$$_sE_R = 2.845\ {}_st_R^{-.483} - .599,$$

which is algebraically equivalent to

$${}_st_R = \frac{8.7}{({}_sE_R + .6)^{2.07}}.$$

3. The relationship of reaction latency in seconds to the number of reinforcements from the very beginning of learning was found to be well fitted by the equation[8]

$${}_st_R = 74.05\ \dot{N}^{-1.60} + .43.$$

4. By a process of extrapolation of the equation involving ${}_sE_R = f(\dot{N})$, a value for the absolute zero of reaction potential was secured. This proved to be[9]

$$Z = -.426.$$

5. Also by appropriate substitution in the equation ${}_sE_R = f(\dot{N})$, a preliminary value for the reaction threshold (${}_sL_R$) was secured. It proved to be

$${}_sL_R = .356\ \sigma.$$

6. The maximum reaction potential, motivation constant, from absolute zero as a base is useful for certain theoretical purposes. By an inspection of one of our fitted equations this value was found to be

$$M' = 3.55\ \sigma.$$

7. The functional relationship or ratio of ${}_sL_R$ to M' was found from the two values just given to be approximately

$${}_sL_R = .10\ M'.$$

8. A little analysis applied to the equation ${}_sE_R = f(\dot{N})$ yields a tentative quantitative centigrade definition of the hab for albino rats in the present simple learning situation. This is

$${}_sH_R \text{ (in habs)} = 1 - 10^{-.0305\dot{N}}.$$

[8] Theoretically, given the three equations (4), (9), and (11), any one of them ought to be derivable from the other two. It must be noted here that the present empirical fits are not precise enough to satisfy this exacting test of consistency. For example, when the mathematical equivalent of equation (9) was derived from equations (4) and (11), an exceedingly unwieldy result was obtained, quite different in appearance from equation (9).

[9] We venture the prediction that the more precise determination which may be expected in the future will increase this value rather than reduce it.

9. The possession of equation (9), coupled with some median latencies published by Perin (9), enables us to arrive at the fitted equation

$$_sE_R = .608 \times 10^{.0148h} + 1.0$$

which, within the range of values available, purports to be a first approximation to one form of the molar law of primary motivation as dependent on the number of hours of food privation.

10. And finally, attention may be called to the fact that the method of quantifying reaction potential upon which the most of the conclusions listed above are based has satisfied the pragmatic test of fertility in the following respect:[10] the methodology for quantifying reaction potential taken jointly with its theoretical setting has yielded a number of equations, several of which may be considered as first approximations to potential primary molar behavioral laws, the determination of parameters involved in such laws, or both. Among such equations may be listed (3), (4), (5), (6), (7), (8), (9), (10), and (12).

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[10] An additional item involving both fertility and consistency with methodological assumptions concerns the use of equation (9) to convert reaction latencies into presumably equivalent momentary reaction potentials. By this means it is possible to plot what purport to be (1) the dispersions both of $_s\dot{E}_R$'s and (2) their paired differences. The rather elaborate incidental computations are not yet complete.

SENSORY PRE-CONDITIONING OF HUMAN SUBJECTS *

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Introduction

The phenomenon of sensory pre-conditioning was demonstrated in an experiment upon dogs reported by the author (1). The animals of the experimental groups were presented simultaneously with the sound of a bell and the light from an electric bulb 200 times. When one of these two stimuli had been made a CS[1] for forelimb flexion by instrumental avoidance training it was found that the other also evoked a flexion response. Animals of the control groups not given the bell and light in combination prior to the conditioning of forelimb flexion to one of these two stimuli, failed to respond or responded infrequently to the other stimulus. The results for the control groups indicate that the response of the experimental animals to the stimulus not involved in the conditioning of forelimb flexion was not due to sensory generalization alone. Therefore the conclusion was drawn that some connection, some sensory conditioning, was established during the presentation of the bell and light and that this connection or sensory conditioning was responsible for the occurrence of the flexion response to a stimulus which had never been paired with a CS or US for flexion.

In a second experiment on this phenomenon reported by the author (2), human beings were used as *S*s, an additional control group was introduced, an additional test of sensory conditioning was tried, and provision was made for relating the verbal report of the *S*s to the occurrence of the phenomenon. This experiment failed to demonstrate sensory pre-conditioning. Of the several differences in the design and conduct of the two experiments, the differences in response and procedure of conditioning appeared to be the critical factors responsible for the failure of the second experiment. The attempt was made to demonstrate sensory pre-conditioning by conditioning the GSR[2] to one of the two stimuli and then measuring the GSR to the other stimulus. Since reliable evidence of condi-

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[1] Abbreviations: US = unconditioned (original) stimulus; UR = unconditioned (original) response to US; CS = conditioned response; CR = conditioned response.

[2] GSR = galvanic skin response.

tioning was not obtained, there was no adequate test of sensory pre-conditioning.

The present report is of a third experiment which demonstrates the occurrence of sensory pre-conditioning in human *Ss*. The design and conduct of this experiment are essentially the same as that of the second experiment, except for the response and the procedure of conditioning it. A complex tone and a light were the sensory pre-conditioning stimuli. Depression of a telegraph key was the response used, and it was conditioned[3] to the light stimulus by instructions of the *E* to the *S*. After practice of the response to light, the tone was presented alone. The light was then given again to provide a measure of sensory pre-conditioning should there have been extinction of the CR to the light by presentation of the tone. There were three control groups. The one for sensory generalization went through the treatment outlined above except for the sensory pre-conditioning trials of tone and light. A second control group was given tone alone in place of the sensory pre-conditioning trials, but no other change was made. The third control group received only training in response to the light with enough additional trials to provide a standard by which extinction to the light might be measured in the other three groups.

Experimental Procedure

The light-shielded sound-insulated experimental room contained a chair and a table. Upon the table was a metal cabinet with a sloping front panel with two symmetrically located apertures three in. in diameter. Attached to the inner surface of the panel and covering the apertures were a milk glass plate which could be illuminated by a seven-watt bulb and a two-in. loud speaker protected by copper wire screening. A set of polished silver electrodes on a leather wrist band were plugged to a jack in the small vertical panel of the cabinet. To the right of the cabinet a telegraph key was mounted rigidly to the table. A multi-conductor cable was led from the cabinet through an opening in the wall of the experimental room to the adjacent control room where electrical connections were made with the control and recording equipment. A two-foot square window in the wall common to the experimental and control rooms contained an x-ray mirror and two plates of clear glass, all of different thickness. With illumination of the control room less than that of the experimental room, the x-ray mirror functions as a one-way visual screen.

[3] The use of instructions to establish the key-pressing response to a light stimulus produces the same end result which might be obtained by procedures commonly described as conditioning. Even though the end result is the same, the differences between standard conditioning procedures and the procedure used in this experiment are sufficiently great to question the advisability of applying the term conditioning to the latter procedure. However, the term conditioning will be applied to the instructional procedure of establishing the key-pressing response to light of the present experiment in order to maintain a uniformity of terminology with previous papers on sensory pre-conditioning. This use of the term conditioning carries no assumption concerning the relationship of the instructional procedure to usual conditioning procedures. The design of the sensory pre-conditioning experiment requires that once two sensory stimuli have been presented simultaneously, one of the two must be made a stimulus for a specific response, so that sensory conditioning may be measured in terms of the response made to the other stimulus, the one not involved in the specific training. For this kind of experimental design, it is unimportant whether the training which produces the performance of a response to one of the stimuli prior to the critical test involves the specific operations described as conditioning or not.

Access to the control room is obtained from a hall into which the door of the experimental room also opens. All apparatus other than that already mentioned was located in the control room. An electric motor which was used as a noise screen masked the sounds produced by the control and recording equipment. The stimuli used in the experiment were the illumination of the two-in. diameter milk glass plate in the cabinet on the table of the experimental room and a complex tone of low frequency produced by activation of the loud speaker in the same box. These stimuli will be referred to hereafter as the light and the tone. The front surface of the *S*'s head was approximately 16 in. from the front surface of the panel of the cabinet. The amount of light reflected from the milk glass plate under general illumination of the room was 9.5 millilamberts. When the electric bulb was activated, the amount of light from the glass plate was raised to 155.2 millilamberts. These measurements were made by means of a Macbeth Illuminometer. The tone was produced by feeding the output of a vacuum tube oscillator into the loud speaker. Keying of the oscillator was accomplished by a click eliminator circuit. The intensity of the tone was held constant and at the approximate locus of the *S*'s head was measured at 43 db. above 10^{-16} watts/sq. cm. by a General Radio Sound Level Meter, Type 759-B, with the weighting control set at the C position. The magnitudes of the light and tone were the same as that in the previous experiment (**2**), for which their equality had been adjusted by the judgements of four observers. A Dunlap chronoscope was used to measure the latency of the key-pressing response. The motor of the chronoscope was driven by the 60 cycle commercial power line. The starting and stopping electro-magnetic clutches were operated by direct current from batteries. Control of the stimuli and the starting clutch of the chronoscope was by means of a synchronous motor timer and relay system. The duration of the tone and light was two sec. The onset of the starting clutch of the chronoscope and of the light or tone, or both, was simultaneous. The *E* manipulated switches for the appropriate selection of stimuli and controlled their presentation by operating a master switch in the circuit of the synchronous motor timer. Latency of the key pressing response or reaction time was read from the dial of the chronoscope and recorded on the data sheet. A pilot light on the *E*'s control box also gave a visual indication of when the *S*'s key was closed.

The *Ss* for this investigation were volunteers from classes in elementary psychology at the University of Wisconsin. Appointments were made in quiz sections for a half-hour period to be spent in a reaction time experiment. Students in the elementary course are motivated to serve as *Ss* in experiments by being given a point toward their final grade for the course. Once having signed up for an experiment, they are motivated to keep their appointments by being docked one grade point if they fail to appear. A total of 168 *Ss*, 86 men and 82 women, were assigned to the one experimental and three control groups by the use of a table of random numbers, so that there were 42 in each group. Data for 33 of the *Ss* were gathered in the spring of 1942 and for the remainder during the academic year 1945–46. The author conducted tests only for 128 of the *Ss*. The remaining 40 *Ss* were tested by an assistant.[4] Separate examination of the data for the two groups by *E* revealed no reason why they should not be combined. Separate examination of the data for the different calendar times showed that the reaction times were not comparable and therefore could not be combined.

The *Ss* of Group I, the experimental group, were conducted to the experimental room, seated at the table, asked to wait a few minutes while *E* adjusted his apparatus, whereupon he would return and give instructions about the experiment. *E* observed *S* through the x-ray mirror and presented the light and tone in combination when the *S*'s head was oriented toward the cabinet on the table. When a total of 10 trials of the light and tone had been given, *E* returned to the experimental room and gave *S* the following instructions:

> This is an experiment in reaction time, as you know. Let's adjust the chair to the table so that you are comfortably seated when your right arm is rested on the table with the two fore-fingers of your right hand on the telegraph key. Press the key a few times and notice that it moves through a very short distance and that it does not need to be pressed hard. Your task is to press the key as fast as possible when the light in the cabinet comes on. You need press it only once and may release it immediately. Be careful that you rest your fingers lightly on the key so that it is not closed when the next light comes. I am going to place these electrodes on your left wrist. The apparatus is adjusted so that you will receive a weak electric shock on your wrist if your reaction time is slow. If you give a quick reaction, you will receive no shock, so respond just as fast as you can. I'll answer any questions you may have about the experiment when it is over.

[4] Mr. Rube Chernikoff.

While these instructions were being given, the appropriate adjustments were made. In cases where *S* appeared not to understand the instructions about operation of the telegraph key, *E* gave a demonstration. Every attempt was made to keep *S* from asking questions. In the few cases where questions were asked, if they referred to the instructions, *E* repeated the instructions; otherwise *S* was told that his question would be answered when the experiment was over.

Upon completion of the instructions, *E* went to the control room and presented *S* with 30 trials of the light, then 10 trials of the tone, and finally 10 trials of the light. Reaction time was recorded for each trial on which response occurred. No electric shock was given with any of the trials. The interval between trials was varied between 15 and 30 sec. except that between the 30th trial of light and the 1st trial of tone, and the 10th trial of tone and the 1st trial of light in the second light series, the interval was 15 sec.

When *E* returned to *S*'s room he asked *S* a series of questions. If *S* had responded to the tone, he was asked why he had responded. If he failed to respond to the tone, he was asked why he did not respond. In either case, he was asked if he expected to receive shock with tone. If no mention was made by *S* of the occurrence of the tone and light together at the beginning of the experiment, *S* was asked whether he was aware of the tone and light in combination and whether he thought this had any influence on his performance to the tone. Finally, *S* was asked why he responded to the series of lights after the tone and whether he expected shock. If *S* had any questions about the experiment, they were answered by *E*.

The *S*s of the control group for sensory generalization, Group II, were put through the same procedure with the following exceptions. The initial procedure of presenting the tone and light together before insturctions were given was eliminated entirely and questions at the end of the experiment which referred to the presentation of tone and light together before the beginning of the experiment were not asked.

The *S*s of the control group for non-contiguous presentation of the light and tone, Group III, were seated at the table in the experimental room and asked to wait while *E* adjusted his apparatus. Ten trials of the tone alone were presented, whereupon *E* returned to *S*'s room and gave the same instructions given to Groups I and II. Thirty trials of light, 10 trials of tone, and 10 trials of light were presented to the *S*s of Gorup III, just as was done for those of Groups I and II. The same questions were asked at the end of the experiment as were asked of Group I, except that the questions about the combination of light and tone at the beginning of the experiment referred instead to the tone alone.

Group IV, the control group for the extinction measures provided by the last 10 trials of light given the other three groups, was instructed immediately upon introduction to the experimental room in the same manner as were the other three groups. This group was given only 40 trials of the light. The interval between trials was varied at random between 15 and 30 sec., except that the interval between the 30th and the 31st trials was 15 sec. At the termination of the experimental session, *E* asked no questions of members of this group, but answered any questions which *S* had about the experiment.

TABLE I

SUMMARY SCHEMA OF EXPERIMENTAL PROCEDURE

Group	Pre-Training	Training	Post-Tests		Questions
I	10 trails light and tone	30 trials light	10 trials tone	10 trials light	See text
II	None	30 trials light	10 trials tone	10 trials light	See text
III	10 trials tone	30 trials light	10 trials tone	10 trials light	See text
IV	None	30 trials light	None	10 trials light	None

Table I summarizes the experimental treatment given each of the four groups. All groups received the conditioning or training procedure. Only Groups I and III were given a pre-training

or pre-conditioning procedure; Group I was given 10 trials of the light and tone; Group III was given 10 trials of tone. All four groups were given post-tests. The post-test for Groups I, II, and III consisted of 10 trials of tone followed by 10 trials of light. The post-test of Group IV omitted the 10 trials of tone, but contained the 10 trials of light.

Quantitative Results

The design of the experiment is such that the occurrence of sensory pre-conditioning in the *Ss* of Group I may be demonstrated by: (1) a greater frequency of response to the 10 trials of tone than that given by the *Ss* of both groups II and III; (2) responses to the trials of tone of shorter latency (quicker reaction time) than the latency of responses of subjects in both Groups II and III; (3) a lesser frequency of response to the 10 trials of light than the frequency of response of the *Ss* of both Groups II and III; and (4) a longer latency (greater reaction time) than the latency of response of the *Ss* of both Groups II and III. All of these measures are indirect measures of sensory conditioning of the tone and the light. For the first two measures the assumption is made that if there has been sensory conditioning of the tone and light, conditioning of the key-pressing response to the light will result in the occurrence of key depression to the tone. The last two measures depend upon this assumption and an additional assumption that the presentation of the tone without reinforcement (lack of shock with a slow reaction or reaction failure and lack of instructions about response to tone) will produce some extinction of the response to light.

Table II presents the number of *Ss* in each group giving each of the 11 possible frequencies of response to the tone.

TABLE II

Number of *Ss* in Each Group Giving Each Frequency of Response to the 10 Trials of the Tone

Frequency of Response to Tone	Group I	Group II	Group III
0	14	18	23
1	13	17	11
2	3	0	0
3	1	1	1
4	0	2	0
5	0	0	0
6	0	0	2
7	0	1	1
8	4	2	0
9	0	0	1
10	7	1	3

To check on the occurrence of sensory pre-conditioning, the mean frequency of response to the 10 trials of tone was computed for the

Ss of Group I, Group II, and Group III. The differences between group means were subjected to the *t*-test in the manner described by Lindquist (4). Table III gives the group means and results of the *t*-test.

TABLE III

DIFFERENCES BETWEEN GROUP MEANS OF FREQUENCY OF RESPONSE TO THE 10 TRIALS OF TONE

	Group II $M = 1.45$ $\sigma_M = 0.49$	Group III $M = 1.71$ $\sigma_M = 0.49$
Group I $M = 3.00$ $\sigma_M = 0.49$	Diff. = +1.55 $\sigma_{Diff.}$ = 0.69 t = 2.25 P = .01	Diff. = +1.29 $\sigma_{Diff.}$ = 0.69 t = 1.87 P = .03
Group II $M = 1.45$ $\sigma_M = 0.49$		Diff. = −0.26 $\sigma_{Diff.}$ = 0.69 t = 0.38 P = .35

The *P*-values for the differences between Groups I and II and between Groups I and III are .01 and .03 respectively. The mean frequency of response of Group I (3.00) is reliably greater than the mean frequency of response of Groups II and III (1.45 and 1.71 respectively) at better than the five percent confidence level. The difference between Groups II and III, with a *P*-value of .35, is not reliable. These results provide reliable evidence of the sensory pre-conditioning of Group I.

The second measure of sensory pre-conditioning could not be applied because the measures of reaction time were not comparable for *Ss* worked with at different calendar times. Calibration runs of the timer, relay-system, and chronoscope indicated that the magnitude of both the constant and variable errors changed with calendar time. The calibration procedure available did not have sufficient precision, and was not used with sufficient frequency to permit the correction of the reaction time measures.

The results by the third method of measurement were completely negative. All *Ss* in Groups I, II, and III responded to each of the 10 trials of light which followed the 10 trials of tone. The frequency measure, therefore, provides no evidence for any one of the three groups of there being extinction of the response to light as a function of the 10 trials of tone.

Because the reaction time measures of the individual *Ss* to the final 10 trials of light could not be pooled for the groups, the fourth measure was applied to the data as follows. The assumption was

made that apparatus conditions were reasonably stable for the interval of time during which the data for a given *S* were gathered. Therefore, for each *S* in Groups I, II, and III, the mean reaction time for the last 10 of the 30 training trials to light was computed, as well as the mean reaction time to the 10 trials of light following the 10 trials of tone. The algebraic difference of the second from the first of these two means was obtained, and the group algebraic means of these differences computed for each group. A similar procedure was applied to the data for Group IV. The mean reaction time for trials 21 to 30 and for trials 31 to 40 of the training trials to light, and the algebraic difference of the second from the first of the two means, were obtained for each *S*. Finally the algebraic group mean of these differences was computed. Table IV presents the mean differences

TABLE IV

GROUP MEAN DIFFERENCE IN REACTION TIME TO LIGHT AS A MEASURE OF EXTINCTION

	Group I	Group II	Group III	Group IV
Diff.	−26.8 ms.	−40.5 ms.	−29.7 ms.	−2.30 ms.
$\sigma_{Diff.}$	4.40 ms.	5.13 ms.	3.56 ms.	3.36 ms.
t	6.09	7.89	8.34	0.68
P	.0000	.0000	.0000	.27

for each group and the results of application of the *t*-test to each of the differences. All differences except that for Group IV are significantly greater than zero at better than the one percent level of confidence. Since the difference in mean reaction time for the 21–30th trials and the 31–40th trials of Group IV is unreliable, the assumption that apparatus conditions were reasonably constant for the period required to run one *S* appears to be justified. Whether or not the group mean differences provide a measure of the effect of the 10 trials of tone upon reaction time to light depends upon the reliability of the differences between the mean group differences. In Table V, all differences between mean group differences are given, together with the results of the application of the *t*-test. The differences between group mean differences of Groups I and IV, II and IV, and III and IV are all reliable at better than the one percent level of confidence. The presentation of 10 trials of tone therefore increased the reaction time to the last 10 trials of light over that given to the last 10 of the 30 training trials to light.

The differences between the group mean differences of Groups I and II, and I and III are such that the extinction measure fails to provide evidence for sensory pre-conditioning of Group I. The increase in reaction time to the last 10 trials of light is reliably greater

TABLE V

DIFFERENCES OF GROUP MEAN DIFFERENCE IN REACTION TIME TO LIGHT AS A MEASURE OF EXTINCTION

	Group II	Group III	Group IV
Group I	Diff. = −13.7 ms. $\sigma_{Diff.}$ = 6.76 ms. t = 2.03 P = .003	Diff. = −2.9 ms. $\sigma_{Diff.}$ = 5.66 ms. t = 0.51 P = .31	Diff. = +26.5 ms. $\sigma_{Diff.}$ = 5.54 ms. t = 4.78 P = .0000
Group II		Diff. = +10.8 ms. $\sigma_{Diff.}$ = 6.24 ms. t = 1.73 P = .04	Diff. = +38.2 ms. $\sigma_{Diff.}$ = 6.13 ms. t = 6.23 P = .0000
Group III			Diff. = +27.4 ms. $\sigma_{Diff.}$ = 4.89 ms. t = 5.60 P = .0000

for Group II than for Group I. There is no reliable difference in the increase of reaction time of Groups I and III. Group II not only shows a significant increase in reaction time to the last 10 trials of light over the increase given by Group I, but also over that given by Group III. The confidence level in both cases is better than five percent. The conclusion may be drawn that the introduction of 10 trials of tone after 30 training trials to light increases the reaction time to a final 10 trials of light less if the tone has previously been presented to the *Ss*, either in isolation or together with the light.

The answers to each of the questions asked at the close of the experiment were examined and classified as follows. For the question "Why did you respond to the tone?" there were four categories: (a) 'startled,' 'accident,' 'all set to respond,' (b) 'tone seemed the same as the light,' 'tone and light occurred together,' (c) 'tone given before the light,' 'was expecting tone,' (d) 'don't know,' 'can't say.' The number of *Ss* in each group who responded to the tone and the number giving a response for each category are given in Table VI. The *Ss* in each group falling within the category

TABLE VI

FREQUENCIES OF RESPONSES TO QUESTION 1

	Number of *Ss* Responding to Tone	Category of Answers to Question 1			
		a	b	c	d
I	28	16	8		4
II	24	17			7
III	29	11		2	6

(a), that of 'accident,' are those who gave a frequency of response to tone of one or two (see Table II). The eight *Ss* in Group I whose answer to

question 1 fell in category (b) all gave a frequency of response to the tone of eight or greater. The two *Ss* of Group III whose answer to question 1 fell within category (c) gave a frequency of response to the tone of nine or greater. Although there is no way of accessing the reliability of the relationship between response to question 1 and frequency of response to the tone, there is no inconsistency with the results between experimental procedure and frequency of response to tone.

The *Ss* who failed to respond to the tone gave answers to the question 'Why did you not respond to the tone?' which were classified as follows: (a) 'instructions to respond to light,' 'no instructions to respond to tone,' (b) 'what tone?,' 'I didn't hear any tone,' (c) 'don't know.' Table VII gives the number of *Ss* in each group giving answers in these categories.

TABLE VII

FREQUENCIES OF RESPONSES TO QUESTION 2

Group	Number of *Ss* Not Responding to Tone	Category of Answers to Question 2		
		a	b	c
I	14	10	1	3
II	18	16	2	
III	23	23		

Answers to the question, 'Did you expect to receive shock with the tone?' were as given in Table VIII. The eight subjects of Group I and two

TABLE VIII

FREQUENCIES OF RESPONSES TO QUESTION 3

Group	*Ss* Responding to Tone				*Ss* Not Responding to Tone			
	Yes	No	Don't Know	Total	Yes	No	Don't Know	Total
I	8	14	6	28	1	7	5	13
II	1	20	3	24		15	1	16
III	2	16	1	19	1	19	3	23

Ss of Group III who said they expected shock with the tone were the ones who gave as their reason for responding to the tone, the occurrence before training of the tone and light in combination and of the tone respectively. These *Ss* also gave frequencies of response to the tone of eight or greater, as did also the one *S* of Group II who reported the expectation of shock with the tone. All of the *Ss* in all groups who gave a frequency of respone of two or less to the tone said they did not expect shock. Some of the *Ss* giving greater frequencies of response said they expected shock, others said they did not.

The *Ss* of Groups I and III were asked if they were aware of the tone and light or the tone while *E* was fixing the apparatus. A total of six *Ss*

in Group I were unaware of the occurrence of the tone and light. Three of the six responded and three failed to respond to the 10 trials of the tone. One *S* of Group III was unaware of the tone in the pre-test and also failed to respond to the later trials of tone.

To the question 'Why did you respond to the light after the trials of the tone?,' the universal response was 'Because the instructions were to respond to the light.' All *Ss* reported the expectation of shock if they failed to respond to the light. A large number of *Ss* in Groups I, II, and III reported that, when the light was presented again after the tone, they expected the tone to appear again either alternately or simultaneously with the light. Many further volunteered that this expectancy made their responses to light slower than were their responses to light beofre the tone was given.

Discussion

The results of the experiment clearly demonstrate the sensory pre-conditioning of human *Ss*. The performance of the subjects in the control groups provides assurance that the greater frequency of response to the 10 trials of tone by the *Ss* of the experimental group (Group I) cannot be accounted for by sensory generalization (Group II) nor by non-contiguous presentation of the tone and light (Group III). Since the experimental group differed from the control groups only by the 10 trials of light and tone in combination, this variable or set of variables must be responsible for the greater frequency of response to the tone when it is presented after the training trials to light. The failure of the other measuring devices to provide positive evidence of sensory pre-conditioning in no way negates the validity of frequency of response to tone as an indicator of sensory pre-conditioning. Apparatus variation made it impossible to find out whether latency of response (reaction time) provides a measure of sensory pre-conditioning. Failure to demonstrate sensory pre-conditioning by means of frequency or latency of response to light after presentation of the tone indicates lack of support for the hypothesis that presentation of 10 trials of tone will extinguish to some extent the response to the light. The slowing of reaction time to light by the *Ss* of Groups I, II, and III after the presentation of the 10 trials of tone is much better accounted for by the hypothesis that presentation of the tone changed the conditions from those of simple reaction time to those for disjunctive reaction time. This latter hypothesis is supported by the verbal reports of the *Ss* of their expectancy that tone might be presented alternately or simultaneously with the last 10 trials of light.

The responses of the *Ss* to the questions asked at the close of the experiment are consistent with the results of their key-pressing performance to the post-test trials of tone and trials of light. The majority of *Ss* in any group who responded to the tone apparently

responded accidentally. These *S*s responded to no more than the first one or two of the trials of tone and in general did not expect to receive shock if they failed to respond. Relatively few of the *S*s in any group responded to the tone with a high frequency. There were 11 in Group I, three in Group II, and four in Group III who responded to eight or more of the 10 trials of tone. The majority of these expected shock if and when they failed to respond to the tone and most of those in Group I gave as their reason for responding, the occurrence of the tone and light together. In view of this, it appears reasonable to assume that these latter *S*s are the only ones in whom sensory pre-conditioning was established definitely. Certainly, their frequencies of response contributed heavily to the value of the mean for Group I and probably account for its being reliably greater than the means of either Groups II and III.

It is quite possible that there are conditions which would produce a much greater sensory pre-conditioning effect both in terms of the number of *S*s in which it is established and in terms of its magnitude. Increase in the number of pre-conditioning trials, the number of training trials, the number of test trials, change in the duration, magnitude, or nature of the pre-conditioning stimuli, or change in the training instructions are examples of such conditions. Additional research must be directed toward the discovery of more efficient conditions for producing the phenomenon before there can be a more precise determination of its nature and essential conditions.

However, the mere demonstration of sensory pre-conditioning may have some significance for learning theory. These experiments permit the inference that some relationship is established between two sets of events, those involved in the presentation of a sound to an organism and those involved in presenting a light to an organism, when the two occur simultaneously for a certain number of times. This relationship is of the kind that is called learned. It may be further specified as sensory conditioning. If this is done, the problem immediately arises as to whether the tone is a CS and the light a US, the light a CS and the tone a US, each is a CS, each is a US, or each is both a CS and a US. If one use the the term stimulus, response is implied. One must assume that any event which is a stimulus to an organism evokes a response of that organism. This assumption leaves one no choice but the last one of those offered in the preceding sentence; i.e., that the tone and the light are US's and also CS's. Sensory conditioning, then, is no different from any other conditioning, except that the experimental operations do not permit specification of the responses involved. Neither the UR to the tone, the UR to the light, the CR to the tone, nor the CR to the light are known, but since UR to the tone and to the light must be

assumed, no difficulty arises in assuming that the simultaneous presentation of the tone and the light for a number of trials results in the establishment of a CR to the tone and a CR to the light. This set of circumstances presents no problem to a learning theory such as that of Guthrie (3) in which the contiguity of responses is assumed to be the significant factor in connecting or establishing either one of the responses to the stimulus for the other. All of the reinforcement or motivation type of learning theories must make additional assumptions concerning the nature and operation of reinforcement or motive, assumptions which are extremely difficult, perhaps impossible, to formulate in this situation. However, the inference of sensory conditioning is dependent upon making the light a CS for an explicit response, key pressing, and then showing that the tone has thereby become a CS for the same explicit response. The instructions to the *S*, the threat of shock, and the presence of the electrodes make possible the formulation of necessary conditions for the occurrence of the phenomenon in terms of reinforcement. Therefore, only the inferred phenomenon of sensory conditioning appears to be critical for learning theory. The experimental conditions of the phenomenon of sensory pre-conditioning are not necessarily critical for any learning theory. However, further experimentation with the phenomenon may provide critical tests for some specific theories of learning.

Summary and Conclusions

1. *Ss* first given 10 trials of light and tone under no instructions give a lever pressing response to tone later when through instruction and practice the lever pressing response has been established to light. That the lever pressing response to tone is due to the pairing of tone and light before training is demonstrated by the reliably lesser frequency of response to tone in the post-test shown by *Ss* in control groups who were given (a) no pre-training (control for sensory generalization) and (b) pre-training with the tone alone (control for non-contiguous presentation of tone and light).

2. The experiment fails to demonstrate the phenomenon of sensory pre-conditioning by an extinction measure involving 10 additional trials of the light for the experimental and three control groups.

3. If the experiment is considered a reaction-time experiment, the introduction of 10 trials of tone, after there have been 30 practice trials to light, significantly increases the reaction time given to a final 10 trials of light. This increase in reaction time is significantly greater if the tone has not been presented in a pre-test (Group II) than if it has (Groups I and III).

4. The verbal reports of the *Ss* are consistent with the results of their lever-pressing performance.

5. The results are discussed in relation to current learning theories but are not considered critical at the present stage of knowledge about the phenomenon.

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SENSORY PRE-CONDITIONING AND INCIDENTAL LEARNING IN HUMAN SUBJECTS

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Introduction

The experimental literature contains numerous reports of cases of learning which take place without reference to any specific motive, instruction, or reinforcing agent. A highly objective and clear example of such incidental learning is the experiment by Brogden (1) on sensory pre-conditioning in the dog. Brogden presented light and sound simultaneously to experimental animals for a number of trials, after which a conditioned foot withdrawal was set up to the auditory stimulus. Following the establishment of the conditioned response to sound, the animals were presented with the light and the response also appeared, although it did not in control animals which had not previously received the sound-light training.

In a second experiment on sensory pre-conditioning using human *Ss*, Brogden (2) attempted to condition the galvanic skin response to one of two stimuli and then measure the response to the other stimulus. He obtained no reliable evidence of sensory pre-conditioning and attributes the difference between this finding and the earlier demonstration of the phenomenon in dogs to differences in the experimental conditions of the two investigations.

The present investigation involves an attempt to obtain evidence of learning in human *Ss* under conditions essentially similar to those used by Brogden with the ultimate aim of understanding the process which makes learning possible in situations where there is an apparent absence of explicit instructions to learn.[1]

Method

Twenty-four college students, none of whom knew the purpose of the experiment, served as *Ss*. They were arbitrarily divided into two groups of 12 *Ss* each.

[1] After completion of the present experiment, the editor of this JOURNAL informed the author of a third experiment by Brogden, the report of which had been accepted for publication (see preceding acticle in this issue). Dr. Brogden kindly permitted the writer a pre-publication review of this paper. In his report Brodgen presents positive evidence of sensory pre-conditioning in human *Ss*. His *Ss* under no instruction were first given 10 trials of light and tone. Later they gave a lever-pressing response to the tone when, through instructions and practice, the response had been established to light. There was a reliably greater frequency of response to tone in this group than in control groups which were given (a) no pre-training and (b) pre-training with the tone alone. Brogden's positive findings are, in the writer's opinion, corroborated by the data presented in the present report despite the differences in the experimental conditions of the two investigations.

Each of the experimental *Ss* was seated in a dark, sound-reducing room before a panel on which was mounted a 100-watt unfrosted electric lamp and an electric buzzer. No other apparatus was visible to the *Ss*. They were instructed to sit in a comfortable position, look at the light, and ask no questions during the course of the experiment. Light and buzzer were presented in combination for two sec. Fifty such stimulus combinations were given at a single sitting, after which the *S* was told when to return for a second session and also cautioned not to discuss his experience with anyone. None of the 12 control *Ss* were given this experience of buzzer and light in combination and, in fact, they were introduced to the experiment for the first time through the procedure about to be described.

Each member of the experimental and control groups was next brought into the laboratory and seated before a table on which was mounted the light-buzzer panel and, in addition, an apparatus of the type described by Watson (6) for conditioning finger withdrawal. The *S*'s middle finger was placed under the tambour with the palm of the hand on the electrode. All of the subjects noted the possibility of shock,[2] but they were told nothing about avoiding it. It remained for them to learn that the shock could be avoided by lifting the finger promptly to the proper stimulus.

After the *S* was adjusted to the conditioning apparatus, the *E* moved into an adjoining room which contained the control and recording devices. Conditioning of the finger withdrawal was begun by presenting the sound 1–3 sec. before the shock. This procedure was continued until the *S* gave the finger withdrawal to the buzzer before the shock for five successive trials. Upon attaining this criterion of conditioning, the *S*, without warning, was presented with the light which had previously been presented to the experimental group in combination with the buzzer during the preliminary period. The light was presented 10 times without shock although the possibility of receiving shock still existed.

Results

Comparative data for the experimental and control *Ss* showing number of trials necessary to attain the criterion of conditioning and the number of responses to the 10 presentations of light alone are summarized in Table I. An examination of these data reveals that

TABLE I

Results Obtained from Experimental and Control *Ss*

(Experimental Group)			(Control Group)		
Subject	Trials to Condition	Responses to Light (Maximum possible, 10)	Subject	Trials to Condition	Responses to Light (Maximum possible, 10)
Male	12	10	Male	11	0
Male	17	0	Male	16	2
Male	21	2	Male	31	4
Male	8	10	Male	13	0
Male	16	6	Male	10	0
Male	22	8	Male	9	1
Female	9	7	Female	13	0
Female	8	0	Female	26	0
Female	16	10	Female	12	0
Female	13	6	Female	12	2
Female	27	6	Female	12	0
Female	11	10	Female	12	0

the experimental group of 12 *Ss* responded to the stimulus never associated with shock a total of 75 times out of a possible maximum of 120 responses. Two of the *Ss* did not respond at all during the 10 test trials. That the responses of the experimental group were

[2] Preliminary tests were made to insure that the shock was adequate to produce vigorous finger movement but not too strong to be endured by the *S*.

not due to generalization is indicated by the performance of the control group, which had not been presented with the light-buzzer combination prior to the conditioning trials. This group responded to the light only nine times out of a possible 120 responses, with eight of the 12 *S*s failing to respond during the 10 test trials.

Individual protocols for the experimental *S*s reveal that responses to light alone occurred almost without exception with decreasing frequency as the test series progressed, except of course for those four *S*s who responded on every one of the 10 test trials. For example, the *S* who responded twice during the 10 test trials gave these responses on the first two trials. Without exception every experimental *S* responded on the first trial if he responded at all.

Of the four control *S*s who responded during the test trials, three of them gave their responses toward the end of the test series. The single exception who responded only once gave this response on the first trial.

There is no evidence that the rate of acquisition of the conditioned finger withdrawal was affected by the preliminary experience of 50 presentations to the experimental group of buzzer and light in combination. The 12 *S*s of this group required a mean of 15 trials to reach the criterion of conditioning, whereas the control group required a mean of 14.7 trials to attain the criterion.

Discussion

Under the conditions of the present experiment an association was apparently formed between the light and buzzer when these stimuli were presented contiguously to the experimental group of *S*s during the pre-conditioning period. This inference follows from the experimental demonstration of finger withdrawal to the light after conditioning of the response to the buzzer through the usual buzzer-shock combination. The significant feature of the demonstration is, of course, the apparent conditioning to the light, despite the fact that this stimulus had never been associated with shock. An essentially similar demonstration on the conditioning of the flexion response in dogs by Brogden (1) leads him to infer that a sensory conditioning occurs between the light and the auditory stimulus. Brogden arrives at no definite conclusion concerning the nature of the mechanism responsible for the association or connection between the light and bell during the period when they act upon the organism without evoking any observable response.

The association between light and buzzer, which apparently occurs with mere repeated exposure without reference to any specific reinforcing agent, seems to be an example of incidental learning. Recently, Postman and Senders (4) have presented the hypothesis

that learning which occurs over and above intentional learning is not incidental in the sense in which the term incidental has been used in the past; viz., implying haphazardness. These writers further point out that specific instructions to learn in a laboratory situation lead to a specific set to learn, but that such a set is only one of several sets to learn that may cause learning to occur. In the absence of instructions from the *E*, the *S* may formulate his own explicit self-instruction.

The hypothesis of Postman and Senders appears to receive some support from the results of the interrogation of *S*s used in the present experiment. At the termination of the experiment, each of the *S*s who had responded to the light was asked the question: "Why was your finger raised when the light instead of the buzzer was presented?" Those *S*s who did not respond to the light were asked: "Why was your finger not raised when the light instead of the buzzer was presented?" Among the experimental group, three *S*s, including one who failed to respond to the light, replied that they had no explanation whatever for their behavior. Eight *S*s who did respond made such comments as the following: "I guess it was because I thought I was supposed to"; "Because there must have been some connection between the light and buzzer"; "I figure there was some relationship between the two when we sat in there the first day"; "You must have intended some kind of hook-up or you wouldn't have shown them together at first," etc., etc. The remaining experimental *S* who did not respond replied, "Oh, I figured it was a catch and wasn't going to be caught lifting my finger at the wrong time."

Among the control group of *S*s, seven of the eight *S*s who did not respond to the light replied: "Why should I lift my finger to a light?"; "Because I wasn't supposed to"; "There was no point to it. I figured you got shocked only when you didn't lift your finger to the buzzer," etc. The remaining *S*s stated that they could give no reason why they did or did not respond to the light. To the writer, these comments imply the existence of an explicit or implicit set or attitude which the *S* formed when first placed in the experimental situation. With the experimental group they had, for the most part, the same effect as explicit instructions from the experimenter would have had to think of light and buzzer together during the preconditioning period. If such a set were operative, it is to be expected that its effects would be manifest immediately upon presentation of an extraneous stimulus. Indeed, this is the case, for without exception, if an *S* responded at all during the ten test trials with light alone, he responded on the first trial. Responses to the light alone which appeared after the first trial may possibly be the result of a condition developed at some time during the 10 test trials instead of a set or attitude formed by the *S* during the pre-conditioning period.

With the control group, the general failure to respond may be attributed for the most part to the absence of any unusual situation, such as the pre-conditioning experience, which would tend to induce the formulation of a positive set to respond to an extraneous stimulus such as the light.

In all forms of the hand withdrawal experiment the question arises concerning the voluntary or involuntary nature of the response. No attempt was made in the present experiment to control for this factor either on the basis of latency or form of response. Earlier experiments by Hamel (3) and Schilder (5) led them to conclude, on the basis of introspective reports of their *S*s and latency of reaction, that the so-called conditioned hand withdrawal was essentially voluntary or at least controlled by mental factors. The results of the present experiment do not invalidate this conclusion and may conceivably support it.

Summary

Human *S*s were subjected to stimulation by a light and buzzer in combination for 50 trials, after which the finger withdrawal response was conditioned to the auditory stimulus by pairing it with an electric shock. Upon reaching the criterion of conditioning, the light was effective in eliciting the finger response in 10 out of 12 *S*s despite the fact that it had never been presented with the shock.

The response to light by the experimental group of *S*s is attributed to an association formed between visual and auditory stimuli during the preliminary pre-conditioning period. This would seem to follow from the behavior of control *S*s, who received no preliminary light-buzzer experience and subsequently did not react in most instances to the light upon its presentation after the conditioning trials.

The hypothesis is advanced that an association was formed between the light and sound during the pre-conditioning period because of an explicit or implicit set on the part of the *S* to relate the stimuli to each other. This occurs despite the absence of any explicit instructions from the experimenter, and is probably operative in most, if not all, cases of incidental learning.

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A COMPARISON OF LEARNING UNDER MOTIVATED AND SATIATED CONDITIONS IN THE WHITE RAT *

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The influence on learning of satisfying acts and their consequences has been one of the most controversial issues in the field of learning. One group of theorists (Thorndike, Hull, Troland, etc.) postulate that certain 'after-effects,' tend to strengthen the S-R connections they follow, while certain other 'after-effects' weaken or have no effect upon the strength of the S-R connections they follow. The selection of the correct responses and the elimination of the incorrect responses result from these differential effects of the various S-R connections.

Opposed to this formulation is the theory of Tolman (7). Tolman does not deny the law of effect as an empirical generalization, for he admits animals do tend to exhibit those responses which lead to satisfying after-effects and abandon those which do not. Nevertheless, he denies that it is an important law (if one at all) of learning. His own theory makes a distinction between acquisition and utilization of learning. Lashley (4) was the first to make such a distinction and it has since been emphasized by others (1, 5). Learning *per se* according to this conception consists in the acquisition of sign-gestalt expectations, namely, cognitions of what signs in the environment will lead to what significates. These acquisitions do not increase the probabilities of specific response tendencies as S-R theorists postulate; they merely supply the organism with a cognitive structure, representative of the stimulus organization of the environment.

As to whether the acquisition of these new sign-gestalt expectations requires the presence of a motivating state and its satisfaction, Tolman tends to answer in the negative. However, in one instance he has formulated a law of motivation, which, approaches the typical reinforcement principle. This law states that:

* This is one of a number of studies on the motivation-reward conditions of learning which were carried out in the Psychological Laboratory at the State University of Iowa. The reader is referred to the initial study by Spence and Lippitt (6).

[1] This article is part of a dissertation submitted in partial fulfillment of the requirements for the degree of Master of Arts, in the Department of Psychology, in the Graduate College of the State University of Iowa. The writer wishes to acknowledge the valuable advice and assistance of Professor Kenneth W. Spence.

If some one of the resultant f's [significates] should be especially satisfying, the learning of the particular sign-gestalt, or even the whole set of sign-gestalts, would probably thereby be facilitated. (8, p. 399)

Such a law is similar to the reinforcement principle of S-R theorists, except that the association is assumed to be between the events corresponding to two stimuli (sign and significate) rather than between a stimulus and a response. However, the findings of the California studies on latent learning, which seem to indicate that acquisition of learning will be just as rapid without motivational satisfaction as with it, tend to make Tolman minimize the importance, if not question, the necessity of such a law.

Leeper minimizes the importance of motivation and its satisfaction on the acquisition of sign-gestalt-expectations. He writes:

. . . as a matter of fact even though we have clear evidence that motivation is required for utilization, we do not have similar evidence to show that without motivation there is no acquisition. Indeed we have good evidence that motivational satisfaction is not required for some learning. . . . (5, p. 6)

Leeper then suggests an experimental design which would answer the question whether motivation and its satisfaction are necessary for the acquisition of sign-gestalt-expectations.

An experiment could be devised which would determine whether greater motivation results in more rapid acquisition on such a learning problem, but the essential point in such an experiment would have to be that, at the conclusion of the training, the two groups would be brought to a state of equal motivation before they were given the crucial tests to determine whether either group had learned more than the other. (5, pp. 6–7)

The following experiment was designed to compare the amount of learning occurring with and without motivation.

Experimental Procedure

The *S*s for this experiment were 32 white rats from the colony of the State University of Iowa psychology department. Their ages ranged from 66 to 99 days at the beginning of training. Twenty-five were females, seven were males.

A single choice T-maze was used. A complete description of this maze is given in a previous paper (3).

All animals were transferred from their breeding quarters to their living cages approximately a week before their initial experience in the maze. Each animal was handled by *E* about three to five min. each day in order to hasten its 'emotional adjustment' to the experimental situation. The preliminary training in the maze was given over a two-day period.

During the first day all doors were raised and the animal was placed in the beginning of the maze and permitted to explore freely for one hour. During this time both drinking panels were covered by their respective guards and no food was in the maze. The animal's initial choice was recorded.

On the second day the animals were given two trials under the same maze condition, with the exception that the starting compartment was utilized. Doors were lowered after animals passed through them, thereby making retracing impossible. The animals' choices were recorded, and if any animal chose the same side three times (including the initial choice of the preceding day), it was discarded. Only one animal was eliminated from the experiment on this basis.

The animals were divided into two groups. The animals (20) of Group M (motivated) were thirsty and hungry simultaneously. They were deprived of both food and water for approxi-

mately 21 hours. The specific details of their regimen are contained in a previous paper (3). The 12 animals of Group S (satiated) were satiated for both food and water.[2] Satiation for food and water was produced by having food and water constantly in the living cages except for the time between the sixth and second hour before the the daily trials. The food and water were removed six hours before the trials and placed back in the cage four hours later. One hour prior to the daily trials small pellets of food (1/30 the size of the biscuits) and a small dish of water were inserted in the cage in order to induce further eating and drinking. This procedure was followed in order to produce as complete satiation as possible. Its effectiveness is revealed by the fact that in no case did any *S* attempt to eat or drink in the maze.

For both groups water was present in one goal box, while food was in the other goal box. In the food box the water panel was always covered. Approximately half the animals of both groups had food in Goal Box 1 and water in Goal Box 2, while for the other animals this situation was reversed.

The training series consisted of four runs daily for seven days. Groups M and S were given the same training series except for the different motivating conditions. The initial run of each day was a free choice, the second was a forced trial to the side opposite that chosen on trial 1, the third was a free choice, while the fourth was a forced trial to the opposite of the choice of trial 3. Thus, the two groups had equal experience with the contents of both goal boxes. Forced trials were accomplished by lowering either of the guillotine-type doors on both sides of the choice point.

The animals of Group M were permitted to eat for 20 sec. in the goal box containing food and allowed to take approximately 10 swallows from the orifice of the water bottle. The animals of Group S were kept in either goal box for 30 sec. Because there were six bicsuits on the floor of the food box, the animal could not avoid touching at least one in its random movements. Since the only indication of the presence of water was the nozzle of the water bottle, the *E* upon removing the satiated animal always rubbed its nose against the nozzle.

The test or critical trials were similar for both groups. They consisted of four trials on four successive days (one trial per day), beginning on the day following the last day of the training series. The maze conditions were similar to those which existed during the training trials. On the first and fourth day of the test series the animals were motivated for one of the goal objects (food or water), while on the second and third day they were motivated for the other goal object.

Since many of the animals of both groups exhibited preferences for one of the two sides, approximately half of the animals of each group were motivated for the goal object on the side they least preferred during the training series. The preferences of the animals were evaluated by their performance during their last four free choices of the sixth and seventh days of training.

Because of the motivational condition of Group S, some difficulty was encountered in their training trials. They were rather sluggish animals because of their satiated condition. Frequently they would stop in the maze. It was decided that a gentle push would be administered if they remained in one spot for more than five min. Fortunately it was never necessary to push any animal while it was in the choice compartment.

Results and Discussion

Table I indicates the mean number of errors made by the animals of both groups.

TABLE I

Mean Number of Errors Made by Groups M and S During Four Test Trials

Group	Mean	Est. σ pop.
M	.60	.82
S	1.33	.78

[2] The term 'satiated' refers only to the condition of the hunger and thirst drives, and not the general drive state of the animals. Since the satiated animals moved in the maze (or were prodded when necessary), the assumption that these animals were completely unmotivated would not be tenable. The important point is that their motivation, whatever it was, did not bear any relationship to the goal objects (food and water). It is in this area that reinforcement theories differ with non-reinforcement theories; the former postulates that a certain relationship must exist between drive conditions and goal objects, for learning to occur, while the latter denies the necessity of such a relationship.

Since the distribution of errors (see Table II) is skewed for both groups, it is necessary to utilize a statistical technique which takes this factor into consideration. A technique devised by Festinger (2) is applicable. With this technique the difference between the two means was found to be significant beyond the one percent level.

TABLE II

NUMBER AND PERCENT OF ANIMALS OF GROUPS M AND S MAKING ZERO, ONE, AND TWO ERRORS DURING THE FOUR TEST TRIALS

Group	0 Errors		1 Error		2 Errors	
	N	%	N	%	N	%
M	12	60.0	4	20.0	4	20.0
S	2	16.7	4	33.3	6	50.0

Table II presents the frequency distribution of errors made by Groups M and S. The difference of 43.3 percent between the animals of each group making perfect runs during the four test trials is significant beyond the one percent level.

TABLE III

PERCENT OF CORRECT RESPONSES FOR GROUPS M AND S FOR EACH TRIAL OF THE FOUR TEST TRIALS

Trial	Percent of Correct Responses	
	Group M	Group S
1	85.0	58.3
2	75.0	66.7
3	90.0	83.3
4	90.0	58.3
Total	85.0	66.7

Table III gives the percent of correct choices for both groups during each of the four test trials. The difference between the total percentages (85.0 and 66.7) is significant at the 2.5 percent level.

According to the most extreme point of Tolman, the acquisition of sign-gestalt expectations is completely independent of motivational satisfaction.[3] A logical deduction from such a viewpoint would be to predict similar behavior for both Groups M and S, since both groups had equal opportunity to acquire expectations as to 'what-led-to-what' during the training trials. The results show that Group M was superior to Group S in their discriminations during the four test trials during which time they had 'states of equal motivation' (defined in terms of the independent variable, hours of deprivation). The results fail to support the contention that learning or acquisition, whether conceived in terms of formation of sign-gestalt

[3] The important problem of whether Group S showed any learning will be discussed in a paper by Spence, Bergmann, and Lippitt.

expectations or stimulus-response associations, is independent of motivation and its satisfaction.

Summary

Two groups of white rats, one motivated by hunger and thirst, the other satiated, had equal experience in a T-maze which contained food in one goal box and water in the other. They were tested under identical conditions of motivation. The group that was motivated during the training trials showed a decidedly superior degree of learning during the test trials. The results supported the view that motivation and its satisfaction are important factors in learning, whether learning be conceived in terms of the acquisition of sign-gestalt expectations or the formation of S-R associations.

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